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No. 14. New Series. April 1880. [2s. 6d.

APR 19 1880

THE POPULAR Science Review

EDITED BY W. S. DALLAS, F.L.S.

Assistant-Secretary of the Geological Society.

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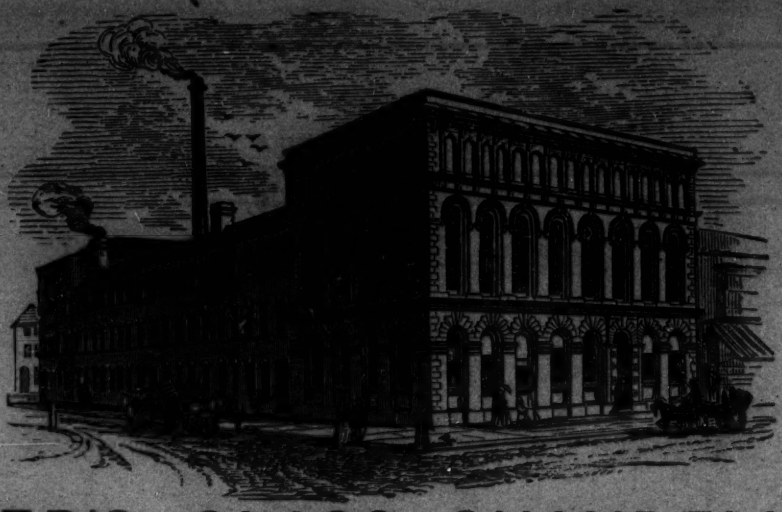

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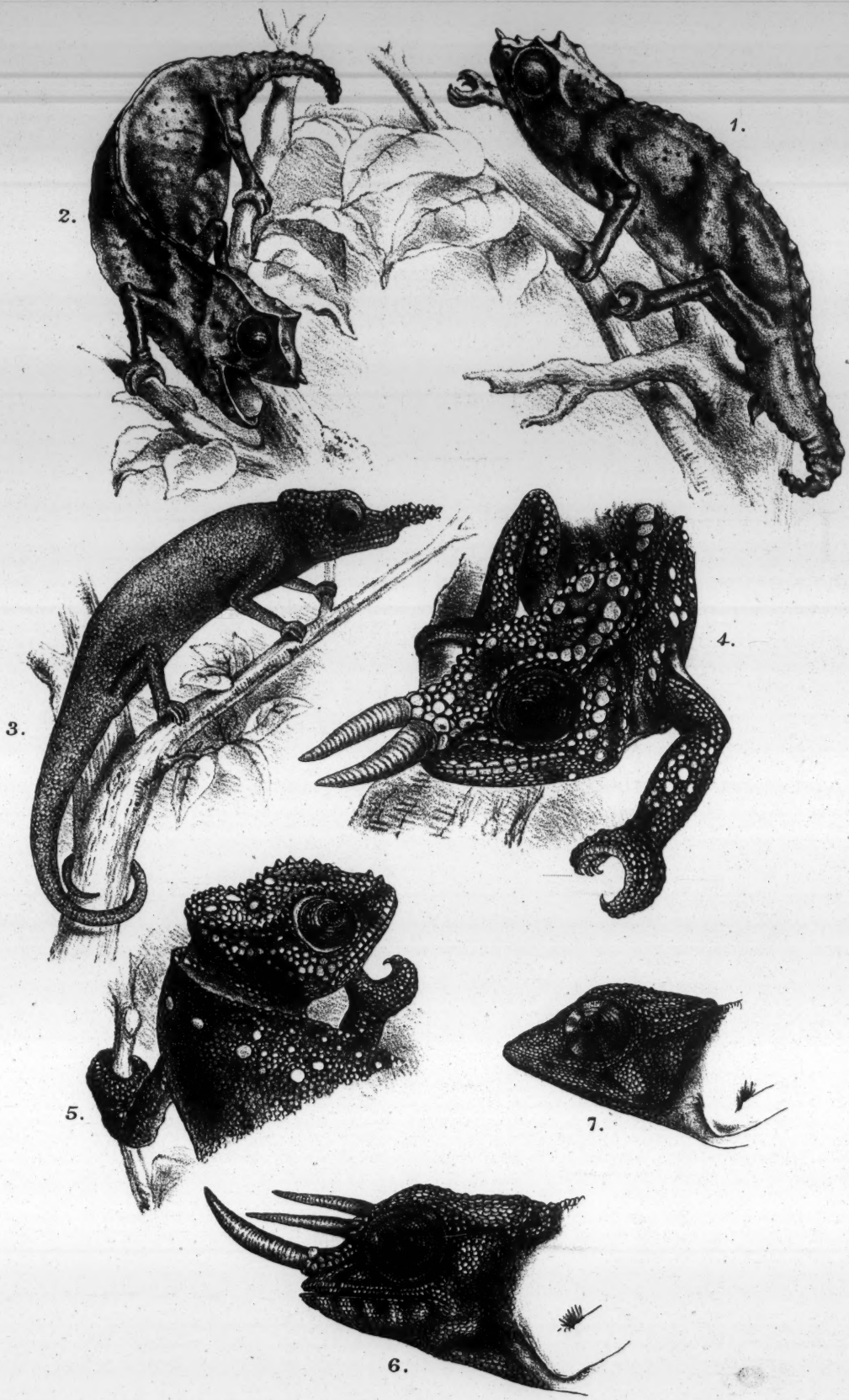

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CHAMÆLEONS.

CHAMÆLEONS.

By Professor J. REAY GREENE, B.A., M.D., F.L.S., F.Z.S., &c.

[Plate III.]

IN the system of nature Chamæleons unquestionably occupy a more conspicuous place than any other family of reptiles now living upon our globe. This family constitutes one of the three sub-orders under which most herpetologists, following Stannius, arrange existing Lizards. On no family of Crocodilians, Tortoises or Snakes can a like dignity be imposed.* The structure of the Chamæleons is, in many respects, very remarkable; their habits, especially those of the common species, are yet more striking. Not even the sloths are so entirely adapted to lead a purely arboreal life. Slower in movement than the tortoises, the common Chamæleon is nevertheless gifted with apparatus for the pursuit and capture of winged prey, which it finally seizes with the most unerring rapidity. Throughout the whole animal kingdom no more notable adaptation of means to end can be said to exist. Such means are at once active and passive; they are numerous and diverse. The long extensile curiously-constructed tongue, the exceptionally mobile eyes with their manifold adjustments, the changing skin, the slender limbs, specially modified for climbing, and the prehensile tail,

* On such questions, as to the systematic value of certain groups of reptiles, there is more or less divergence of opinion. Thus Agassiz would have separated the turtles, as a sub-order, from the remaining tortoises, whilst some have suggested a like separation of the family of geckos from other lizards. But these views have not been generally accepted. Again, the typhlopine serpents differ much in the structure of their skull not only from other serpents but from reptiles in general. Yet in the judgment of Johann Müller, who is here followed by most modern anatomists, these remarkable snakes do not constitute a group of higher rank than that of a family (Typhlopidæ). Among tortoises and batrachians, also, very notable differences as to cranial structure may exist without being accompanied by corresponding diversities, at all comparable in extent or apparent importance, in other parts of their organization. Extinct reptiles, such as Ichthyosaurus, are excluded from this comparison.

terminating a carcase unparalleled for meagreness, are not the only parts of the Chamæleon which demand attention.

It is true that a rank equal to that usually conferred on the Chamæleons might be awarded the singular Hatteria (*Sphenodon* or *Rhynchocephalus*) of New Zealand, whose characters have been so well described by Dr. Günther.* It, too, recedes from the typical lizards, while it approaches the crocodilians and other reptiles. Its nearest affinities are less with recent saurians than with certain long extinct members of its order. In so far as it is an aberrant, it is also, for the most part, a generalized lizard, resembling lower rather than higher forms. The Hatteria deceives us, for its outward aspect gives little clue to the solution of the riddle propounded by its deeper anatomical peculiarities. For this reason it was at first erroneously classed with the Iguanas, to some of which in habit it sufficiently approximates. But the ways of the Chamæleons, no less than those morphological features which yield so much delight to the pure anatomist, are at once seen to be very exceptional and worthy of note by the ordinary observer. Thus, whether we consider their structure or their mode of life, these reptiles may fairly claim the isolated position commonly assigned them.

Does the sub-order and family of Chamæleons include more than one genus? The late Dr. John Edward Gray, who, during his later years, would seem to have felt a passion for the subdivision of old genera and the institution of new ones, has endeavoured to establish no less than fourteen genera in place of the one usually admitted. It is difficult, if not impossible, to agree with this author's estimates. His thirteen new genera rest on characters which are either paltry in themselves, and perhaps not always of even specific importance, or taken from one sex only. Rightly to classify Chamæleons, we need very extensive suites of specimens preserved in fluid, collected from as many localities as possible, and accompanied by the notes and drawings of intelligent travellers.

A much more competent authority, Dr. Günther, distinguished among living zoologists for his knowledge of the species and genera of cold-blooded vertebrates, has lately proposed a new genus of Chamæleons, which we have no choice but to admit for the present. This genus, *Rhampholeon*, contains one well-marked species (*R. spectrum*), whose strange characters are manifest at a glance (see Pl. iii. figs. 1, 2). It is a small species, with an exceptionally short tail. The male has a total length of thirty-nine lines, the tail being thirteen; the female is thirty-five lines, with a tail nine lines in length.

'The tail is so short that it can serve as a prehensile organ

* See his memoir in the *Philosophical Transactions* for 1867.

in a very subordinate manner only. This defect is compensated by the development of an additional sharp denticle at the inner base of each claw, and of a spine vertically projecting from the flexor side of each finger and toe, which must immensely strengthen the power of the animal for holding on to branches.'*

Dr. Gray's list,† published in 1864, enumerates thirty species of Chamæleons (or an average of two species to each of his genera). More than a dozen others have since been added.

It is very necessary to note, in our study of the variations to which the species of Chamæleons are subject, the secondary sexual characters which they display more strikingly than any other reptiles. Mr. Darwin,‡ in his account of selection in relation to sex, thus treats this division of his subject:—

'In the genus Chamæleon we come to the climax of differences between the sexes. The upper part of the skull of the male *C. bifurcus*, an inhabitant of Madagascar, is produced into two great, solid, bony projections, covered with scales like the rest of the head; and of this wonderful modification of structure the female exhibits only a rudiment. Again, in *Chamæleon Owenii* from the West Coast of Africa, the male bears on his snout and forehead three curious horns, of which the female has not a trace [see Pl. iii. figs. 6, 7]. These horns consist of an excrescence of bone covered with a smooth sheath, forming part of the general integuments of the body, so that they are identical in structure with those of a bull, goat, or other sheath-horned ruminant. Although the three horns differ so much in appearance from the two great prolongations of the skull in *C. bifurcus*, we can hardly doubt that they serve the same general purpose in the economy of these two animals. The first conjecture which will occur to every one is that they are used by the males for fighting together; but Dr. Günther, to whom I am indebted for the foregoing details, does not believe that such peaceable creatures would ever become pugnacious. Hence we are driven to infer that these almost monstrous deviations of structure serve as masculine ornaments.'

Here we may refer to Ford's beautiful figure§ of *C. Melleri*. The snout of *C. gallus*, a small Madagascar species (Pl. iii. fig. 3), 'has a long pointed, flexible appendage, which is covered with large soft tubercles.' In both these species the male only is known, as in the no less curious *C. malthe*, *C. brevicornis*, and *C. globifer*.‖

* *Zoological Society's Proceedings*, 1874, p. 442 and Pl. lvii.

† In his *Catalogue of Lizards*, published in 1845, only eighteen species of Chamæleons are mentioned. See *P. Z. S.*, 1864, p. 465.

‡ *Descent of Man*, vol. ii. 1871, p. 34.

§ *P. Z. S.*, 1864, Pl. xxxii. It accompanies Dr. Gray's *Revision*.

‖ Chamæleons from Madagascar, described and figured by Dr. Günther in *P. Z. S.*, 1879, Part I.

Doubts as to the limits due to variation must check our statements touching the geographical distribution of the Chamæleons. We have not, hitherto, been able to reject many of the reputed species. Of others the precise range remains to be ascertained. Many seem local (confined to restricted areas).

We know no good species which does not inhabit Mr. Sclater's Ethiopian region.* The common Chamæleon is found in Southern Africa, and is, moreover, the only well-ascertained ultra-Ethiopian species. The number of continental is about equal to that of insular species. But few species cross the equator, though the number of such species will probably be increased by the researches of future collectors. Certain it is that south of the equator Chamæleons are more numerous.

Southern (extra-tropical) Africa has six species, besides *C. vulgaris*. Four are peculiar, *C. ventralis* and *C. pumilus* from the Cape, *C. namaquensis* from Little Namaqualand, and *C. melanocephalus* from Port Natal. Two others, which occur at Port Natal, are also tropical, *C. dilepis* from Gaboon and *C. nasutus* from Madagascar. At least a dozen species, in addition to *C. dilepis*, belong to western tropical Africa. Fernando Po has two peculiar species (*C. Burchellii* and *C. Owenii*) and one common to it and Old Calabar. The other western species are continental. *C. gracilis* has the greatest meridional range, extending from Senegal to Angola. South of the equator we also find *C. Capellii* from Benguela and *C. anchietæ* from Mossamedes. On this side of Africa the species seem more numerous north of the equator. The Camaroons yield *C. montium* (Pl. iii. figs. 4, 5) and *Rhampholeon*, the most aberrant of all the Chamæleons. Lastly, *C. Brookesii* quits western Africa, reappearing in Madagascar!

From eastern tropical Africa we have four continental species, *C. lævigatus* from Khartoum, *C. affinis* from Abyssinia, *C. Petersii* from Mozambique, and *C. Melleri*. The first is, probably, further from the coast than any other tropical species. Of insular species, all south of the equator, twenty-one are peculiar, namely, from

Madagascar	15	species.
Madagascar and Bourbon	3	„
Madagascar, Bourbon and Mauritius	1	„
The Comoro Islands	1	„
The Seychelles	1	„

Madagascar has a total of twenty-one species; but two of these are also continental. According to Böttger, Chamæleons make up one fourth of the saurian fauna of this island, which we know to be equally peculiar as to its mammals. Five new

* Not including Northern (extra-tropical) Africa, which, with part of Asia and Southern Europe, belongs to the Mediterranean sub-region.

species of Chamæleons from Madagascar have been described by Dr. Günther since Böttger's estimate was made. In no other region is the genus Chamæleon so conspicuously represented. It is curious, if true, that the tropical African mainland nearest to Madagascar should be poorer in species than the western coast. Is this due to its greater humidity,* or have we here to deal with an effect of migration, as in the case of the singular reptilian fauna of the Galapagos Islands? †

We find little or no mention of Chamæleons frequenting central Africa properly so called.

Omitting the Cape species and dividing the Ethiopian region by its principal meridian, that of 20° E. longitude, we find only two species of Chamæleons which live on both sides of this line.

The limited distribution of the Chamæleons and the fact that none are known to be extinct ‡ indicate the lateness of their origin. Pliny has called Africa the land of wonders in a sentence approvingly quoted by Linnæus. Professor Owen, who cites the same passage, has shown that its reptilian fauna in past times was no less wonderful than in the present.

We now return to the common and famous species, which also enjoys by far the widest range. It occurs in Spain (Andalusia), northern Africa, southern Africa, Asia Minor, the Indian Peninsula and the northern parts of Ceylon. The British Museum contains specimens said to have been brought from Singapore and even Japan. Its presence in Ceylon has recently been denied, but the rebutting evidence on this point is indisputable. The occurrence of the Chamæleon in Sicily has been asserted, denied, and re-asserted.

Does the common Chamæleon (like *C. Brookesii*) belong to the list of what Alphonse de Candolle has termed 'disjointed' species? Thus, we find it recorded from northern and southern (but scarcely from intertropical) Africa. This alleged distribution plainly suggests that of the many African species one or more may be varieties of this common form. Have we not here

* For 'west coast' read 'east coast' in paragraph (275) of Sir John Herschel's article on 'Physical Geography,' in the last edition of the *Encyclopædia Britannica*.

† See chap. xvii. of Darwin's *Naturalist's Voyage*.

‡ We do not forget the fossil found at Wyoming of which the following account has been published:—

'CHAMELEO PRISTINUS. Indicated by a lower jaw fragment containing eight teeth in a space of five lines. In every respect it agrees in character with the corresponding part in living species of the genus.'

So noteworthy a conclusion as the existence of Chamæleons in North America during Eocene times must rest on fuller evidence than this passage affords.

a case somewhat similar to that offered in botany by the Cedar of Lebanon, with its western and eastern outliers, the Atlantic Cedar and the Deodara? Again, Dr. Gray cites no localities intermediate between Asia Minor and Hindostan. The Chamæleon of our childhood, from 'Arabia's wilds,' as narrated in Merrick's poem, is not, according to Dr. Gray, the common form but a distinct species, *C. auratus*. Such questions should not be undecided.

The Chamæleon is often mentioned but little cared for by the vulgar, who regard the creature with the misplaced wonder of contented ignorance rather than with the intelligent curiosity which it deserves. It can hardly be called a favourite, though among the cold-blooded vertebrates there is no other animal so well fitted to be made a household pet. Our knowledge of its structure and actions is still far from complete, yet very many naturalists have studied it. A long list of essays specially devoted to its elucidation might easily be cited; and many allusions are made to it in more general works, with titles which would scarcely lead us to expect such references. Two lines of inquiry need to be followed up, that we may trace what remains to be ascertained of the Chamæleon's nature and history. First, its several parts, the eye only excepted, have not hitherto been minutely analysed with those modern aids to research which are now so accessible. In the second place, the functions of its muscular and nervous systems have never been duly investigated by competent physiologists, availing themselves of the resources of experimental methods.* Much might be learned in this way, even though we should curtail our studies from an unwillingness to subject the living animal to pain. Therefore, the life of the Chamæleon, as contemplated by men of science, still remains in many respects a mystery.

The Chamæleon may from time to time be bought and kept in captivity. Care should be taken to supply it with plenty of flies, crickets or such other insects as can be had. (A fly-trap of honey or syrup may be used to save trouble.) It should be lodged in a properly ventilated glass case, some thirty inches in length breadth and height, furnished with a suitably branched shrub or fragment of a tree. A warm temperature should be maintained about it. Lastly, it should be allowed to relieve its thirst. The necessity of so doing is well shown by Brehm,† who carefully studied Chamæleons when residing in Alexandria. He procured during the summer a large number of healthy specimens, more than a third of whom died after a fortnight's captivity. The remainder were very languid, of a dull uniform

* Some use, it is true, has been made of these methods in the study of the Chamæleon's changes of colour.

† See his *Thierleben*, seventh volume of second edition, 1878.

grayish yellow colour, and careless of the food abundantly supplied them. Brehm now tried the experiment of treating his pets to an artificial shower of rain. Like magic, they revived. Their skin reassumed its more vivid and changing tints; they moved their sluggish limbs, going from leaf to leaf in quest of the grateful moisture, and, displaying with increased energy their insatiable greed for prey, soon appeared to be in better health than ever.

Those who have seen Chamæleons in life would laugh at us for attempting to describe their form. Those who cannot view the living animal will learn more from the excellent (though uncoloured) figure of a group of Chamæleons in the work of Brehm than from any written description or even, we might add, from the inspection of preserved specimens. We must, however, say something which may induce our readers to study these animals more closely, and we make, therefore, the following attempt to represent the Chamæleon's most characteristic features.

The head is large, compared with the rest of the body, and though relatively short, is wide and very deep. In general form it is angular, with a high occipital crest, from the raised hinder apex of which a ridge-like wing descends on either side, then arches over each orbit, and finally stretches forward to meet its fellow just behind the rather blunt muzzle. The gape is very wide, the under jaw capacious. Ths nostrils lie far forwards, but are not very close. There are no external ears.

The neck is short and stiff, appearing externally as little more than a fold between the head and the narrowed shoulders, from which the whole body slopes gradually backwards.

The trunk proper, compressed laterally, is singularly lean, and is sharpened along the middle line both of the back and belly, the ventral ridge being continued on the chin.

The tail also is much compressed, but is rounded beneath. It is prehensile, and usually twisted round some support. It constitutes more than half the total length of the animal, which is about ten inches.

The slim nearly cylindrical limbs, not swollen in any part and much longer than the trunk, remind us of those of Cassius or Don Gonzales Pacheco. They end in hands and feet stouter than themselves, with their digits so arranged as to grasp securely the branches on which the animal rests. In each hand, the thumb, index and middle fingers, united by a membrane as far as their nails, are directed inwards; while the two other fingers, likewise united, are turned outwards. In each foot, the first and second toes are connected and turned inwards;

they are opposed to the three outer toes, connected in the same way.

The Chamæleon does not grovel like other reptiles. Its hips and shoulders are so disposed as to allow the limbs to sustain the trunk at a notable height above the branch which supports it.

When not pursuing its prey the Chamæleon maintains an almost corpse-like stillness. But its rapidly shifting eyes, moving independently of each other and often glancing in different directions, contribute much more than the creature's changes of colour to enhance the strange weirdness of its aspect. Nor is this effect lessened, when it begins to stir, by its very stealthy and deliberate movements. Having sufficiently neared its prey, it pauses while it takes aim; and the body in general is motionless as the tongue escapes from its mouth with incredible velocity.

These strong contrasts of motion and rest, its painfully gaunt form unrelieved by any amount of gluttony, and its marvellous fitness to do the work of its life are the chief sources of our interest in the Chamæleon. As a fly-catching machine it is perfect. What seem defects in its organization are truly the reverse. We repeat that the Chamæleon's feebleness, rightly understood, must be regarded as operating in its favour. If it moved its legs quicker, its eyes or its tongue slower, it could not secure its prey with such fatal accuracy. We have seen ten minutes elapse between the first sighting and the final capture of a large bluebottle-fly by a captive Chamæleon. During this protracted interval, as the animal with persistent caution stole gradually upon its victim, alternately raising and putting down one leg after another, no doubt of its ultimate success could be entertained.

In Merrem's arrangement of lizards the Chamæleons constitute the group of *Prendentia*, on account of their grasping limbs. They are more frequently named *Vermilingues* or *Rhiptiglossi*, from their peculiar tongue.

The Chamæleon's skin is, for the most part, not scaly in the ordinary sense, but rather soft and extensible. Small distinct tubercles of uniform size serve to strengthen it. The dorsal ridge is minutely serrated. Along this and the ventral ridge, as well as on the head and limbs, the tubercles are closer, flatter and more scale-like.

The common use at all times of the word Chamæleon in metaphor, its application by the ancients to certain plants and by the moderns to a well-known mineral, show that the changes of colour to which it is subject have attracted much more attention than its other peculiarities. In this respect, however,

the Chamæleon enjoys a pre-eminence which it scarcely deserves. For many lizards and other animals possess the same property, if not in the same degree. We may mention more particularly the cuttlefishes among invertebrates, and among true fishes the sticklebacks. A large number of similar instances might be cited.

We must here distinguish changes due to the presence of special pigment-cells, or 'chromatophores,' from diversities of colour depending on true variation, sex, age, seasons of the year (notably in animals which moult) and the more transient results of altered states of the circulation, as in blushing or the reverse.

When chromatophores are present they are affected by physical influences, such as light, heat, contact of foreign bodies, and other varying external conditions; they are also subservient to emotional causes.

Chromatophores occur in several reptiles, batrachians, fishes, mollusks, crustaceans and insects. They are deep-seated tegumentary cells which contain a dark pigment. According as they approach or recede from the surface of the skin, they modify the tints of the semi-transparent more external layers.

But how is this movement effected? It is not the result of muscular action, as some have supposed, for the skin has no muscles which can thus rearrange its pigment-cells. We hold the opinion of Leydig, who is supported by von Siebold, that the chromatophores are themselves capable of contraction. The contractile substance would seem to be situate chiefly in their outer portions. The pigment itself is not diffused but granular. It has no inherent contractility, however it may be disposed in relation to the true contractile substance.*

That the nervous system governs the movements of the chromatophores may be admitted. Bert, who has made experiments on Chamæleons to settle this question, asserts the presence of two distinct sets of nerves, by which the alternating changes of the chromatophores are directly excited. He even assigns to these nerves their appropriate centres. But these experiments should be repeated. Perhaps they prove too much, for we need more positive evidence to disprove the hypothesis that the retrocession of the chromatophores is essentially a passive movement.

The opposite sides of the same Chamæleon may be differently coloured. Dead and sickly Chamæleons are paler than others. So, likewise, are Chamæleons kept in darkness. We cannot follow Bedriaga in his efforts to ascribe the dark tints

* Harting has made the surprising statement that the peculiar pigment-cells of a cuttle-fish (Loligo vulgaris) have a coloured wall enclosing a cavity filled with soft contractile protoplasm.

(whether temporary or permanent) of these and other lizards to the sole influence of light, acting directly on the deeper cells of the skin and causing them to assume a more superficial position. Leydig has sufficiently disposed of this explanation. Our space does not allow us to cite the details of his very interesting and convincing observations.

All the actions of the Chamæleon, except during the breeding season, tend to the procuring of food. The tongue is the prehensile organ used for this purpose. A Chamæleon projecting its tongue may truly be designated the entelechy, or perfection, of the living animal.

When not thus employed, the tongue lies concealed in a special depression of the floor of the mouth. We must therefore distinguish its retracted from its extended condition. Brehm rightly indicates a third, or intermediate, phase, in which the tongue, previous to being ejaculated, is loosened and pointed towards its prey.

The tongue consists of two principal regions, a proximal and a distal. The former, or so-called 'worm,' is exceedingly extensile, but is much shortened in the retracted condition. The latter is terminal and club-shaped, preserving in its alternate states nearly the same dimensions. Milne-Edwards distinguishes a basal division, behind the worm, which may rather be said to consist of the hyoid muscles. These are very complicated. A full technical description of them would be out of place in the present paper.

The worm is not inaptly so termed, whether we view it in its retracted or extended state. In the former, it is much corrugated transversely, and is about as long as the 'club;' the whole tongue having an average length of an inch and a half. When fully stretched out, to an extent of some five inches, the worm is nearly smooth. A trifling fold still distinguishes it from the club, into which it passes by a gradual enlargement.

The club is bilobed, with an upper and a lower 'lip.' The extremity of the latter projects in front of the tongue, and its ventral aspect is plainly more elevated than that of the club lying behind it, from which it is marked off by an indentation. The upper lip, or 'dome' of Mr. Salter, is itself shaped like a tongue in miniature, having a raised dorsal surface, the gently narrowed hinder portion of which descends rather abruptly to join the club proper. From this part to its anterior end the dome is more than half as long as the club. The shallow fissure between the two lips is dilated into a funnel-shaped cleft by their separation when the tongue is thrown forwards.

A glutinous secretion lubricates the club, which has minute

glands of its own. But other glands moisten the tongue, as it lies in the recess of the lower jaw.

The tongue is a tube, the axial cavity of which is occupied by a slender cartilage, the glosso-hyal or ento-glossal, supporting nearly the whole length of the organ in its state of rest. The soft parts of the tongue consist of (1) epithelium; (2) pigmentary and submucous tissue separated by loose interstitial connective tissue from a fibrous sheath, and (3) longitudinal muscles, besides nerves glands and vessels. There is also an inner fibrous sheath with a smooth free surface, which glides over the glosso-hyal cartilage. The club has peculiar muscles, well described by Zaglas. The existence of minute smooth intrinsic muscles within the substance of the worm is still disputed.

It is probable that four sets of muscles successively promote the extension of the tongue, namely, (1) certain of the hyoid muscles, (2) the longitudinal muscles, (3) the smooth muscular fibres of the worm, and (4) the muscles of the club. Other hyoid muscles retract it. To the same muscles, however, different uses have been assigned by different observers. It has even been conjectured that the extended condition should be regarded as the more passive state of the organ. Others consider the protrusion of the tongue as a sort of erection; but such vascular turgescence must here play quite a subordinate part. Nor can the mere elasticity of the sheath effect much. We cannot here discuss these diverse explanations. The whole subject demands fresh researches. As Milne-Edwards has well said, the mechanism of this movement has not yet been explained in a satisfactory manner.

Though very carefully aimed, the tongue darts from the mouth as if its previous training thoroughly suited its own spontaneous energy. Perrault erroneously supposed that it was coughed out by air driven into it from the lungs. Bibron has graphically compared its expulsion to the act of spitting. Pagenstecher, on one occasion, saw a Chamæleon eject its tongue with such force that the animal lost its hold in consequence, and tumbled off the tree on which it was resting.

From the tongue of the Chamæleon we naturally pass to its eyes. For by these the prey is first perceived, and if we could trace the intermediate changes which take place in the nervous system we should then be in a position to understand how the tongue receives the orders to do its work.

The eyes are relatively large. Each as a whole is nearly spherical, the equatorial slightly exceeding the axial diameter ($8\frac{1}{2}$: 8 millimeters). But what may be termed the morphological equator of the eye, corresponding to the border of the

sclerotic in front of the retina where the corpus ciliare and ciliary muscle are placed, is of no great extent. It gently rounds off into the region behind it, in a manner not conspicuous externally. The anterior portion of the eye is more strongly prominent.

The basal moiety of the sclerotic is exceptionally small. It is constituted by a round cartilaginous disc overlain by a layer prolonged from the much larger fibrous portion. This disc does not reach the optic nerve. The anterior zone of the fibrous moiety, surrounding the lens, is strengthened by a ring of peculiarly curved thin bony plates which are sunk in its substance.

The choroid is very thin, but is notably thickened posteriorly throughout the region subtending the yellow spot. A somewhat conical projecting pecten, about a millimeter in depth, covers the place of entrance of the optic nerve. The corpus ciliare is broader than the iris and pupil taken together. The muscles of the iris are extensive, as in birds; from the marginal sphincter fibres stretch backward towards the ciliary border, and behind these is a less powerful radial dilatator. The iris of the dead Chamæleon is half as wide as the pupil. It lies in complete contact with the lens. Its brilliant anterior surface has a dark background. Insignificant prominences replace the ciliary processes, anterior to which occur exceedingly weak meridional flutings. On the outer side of the corpus ciliare the ciliary muscle (*tensor chorioideæ* of Brücke) arises from a conspicuous lamina of connective tissue, which reflects light and serves to support the cornea. The relative diameter of the cornea is less than in any other vertebrates except the turtles.

The anterior chamber of the eye, containing the aqueous humour, is remarkable for its slight depth.

The lens, on the other hand, is strongly convex.

But of all parts of the eye the retina is most worthy of study, because of its large yellow spot. This, the region of exact vision, occupies the hinder pole of the retinal concave, and displays a central pit surrounded by a far extending zone, throughout which the retina is much thicker and beautifully modified in its minute structure. Especially modified is the so-called percipient layer.

The corresponding region of the human eye shows this layer to be furnished in the fovea proper with cones only. The yellow spot around it has many cones and but few rods, while in the remainder of the retina the rods greatly outnumber the isolated cones. Of two equal retinal areas that which has more numerous (and therefore more slender) cones permits more precise visual discrimination. Heinrich Müller dwells on the

following points of difference between the percipient layer of the Chamæleon and that of man.

1. The Chamæleon has no rods, but cones only.
2. The foveal cones in the Chamæleon are remarkably thinner than in man.
3. The (absolute) length of the foveal cones in the Chamæleon, notwithstanding its smaller eyes, is more conspicuous than in man.
4. The difference in the diameter of the cones, both in the peripheral and central regions of the retina, is greater in the Chamæleon.
5 The region corresponding to the human yellow spot is more extensive in the Chamæleon.

On the whole, sums up H. Müller, if we compare the human eye with that of the Chamæleon, the reptile has altogether the advantage.

Outside the bulb of the eye, but within the large though shallow orbit, the optic nerve, which is here remarkably long, makes a complex loop. It bends downwards, outwards, and again upwards (or even inwards, in certain positions of the eye), previous to its insertion.

Retractor muscles of the eye appear to be absent.

There is a large Harderian gland at the anterior angle of the eye, although the nictitating membrane is rudimentary. The lachrymal gland is small.

The two eyelids of man are represented by one great circular curtain, drawn over nearly the whole periphery of the protruding bulbus and circumscribing a small central orifice. A bony plate lies in the lower moiety of this lid. Beneath the skin of the lid, which resembles that of the general surface, is spread an extensive orbicularis muscle.

The free surface of the conjunctiva, very distinct from the adjoining lid, is also of unusual extent, reaching as far back as the equator of the bulb. The extraordinary mobility of the Chamæleon's eyes, in which it far surpasses all other vertebrates, is much aided by this arrangement, to which the curious curvature of the long optic nerve also contributes.

Thus, whether we consider the eye itself or its appendages, we have to do with an apparatus which is without parallel in the animal kingdom.

The male of the common Chamæleon differs but slightly from the female. He is known by his occipital crest, which is longer and higher, and by the shorter fold occupying the region of the neck.

The female lays a heap of sphæroidal eggs, grayish-white in tint, with calcareous, very porous shells. Oviposition has been

observed only in captivity. No one seems to have witnessed the hatching of the eggs, or to have determined the period of incubation. Brehm found that many females, 'even the strongest and healthiest,' die before or soon after the breeding season is over. He gives an interesting extract from Vallisneri, who noticed that one of his captive Chamæleons was for some days restless on her perch. Thence she slowly descended, moved about for a while, and at length paused in a corner of the floor of her box, which was covered with hard earth. In this she made a hole with one of her forepaws. For two days she worked, digging a pit about ten centimetres wide and fifteen deep. In this pit she now laid more than thirty eggs, and then, as she retired, pushed back the earth with her hind-feet, treating the heap of eggs as cats do their dung. Finally, she covered the heap with straw, dried twigs, and withered leaves.

That the Chamæleon bears living young is often untruly said. It is well known that in many so-called viviparous reptiles the egg is detained in the oviduct and there developed. What is thus normal in these may possibly occur as a rare (or pathological) exception in the Chamæleon. But full proof is wanting.

EXPLANATION OF PLATE III.

(*All the figures are of the natural size.*)

FIG. 1. *Rhampholeon spectrum*, Günther, from the Camaroon Mountains. Male.

FIG. 2. The same. Female.

FIG. 3. *Chamæleon gallus*, Günther, from Madagascar. Male.

FIG. 4. Head of Male *Chamæleon montium*, Günther, from the Camaroon Mountains.

FIG. 5. The same. Female.

FIG. 6. Head of the Male *Chamæleon Owenii*, from Fernando Po.

FIG. 7. The same. Female.

THE NEW CHEMISTRY, A DEVELOPMENT OF THE OLD.

By M. M. PATTISON MUIR, M.A., F.R.S.E.

IN a paper published in this *Review* (January 1878), I endeavoured shortly to summarize the more important differences between that system of chemistry which was founded on a so-called equivalent notation, and the modern, or atomic phase of the science.

The general conclusion to which that summary led was, that the old chemistry was empiric, whilst the new is scientific; but, as was there remarked, empiricism precedes science: science is the natural development of empirical statements, and is not to be regarded as entirely a new departure.

Believing, as I do, that the old and new chemistry are essentially opposed in their methods, I nevertheless am certain that the germs, at least, of many of our modern chemical theories are to be found in the statements, and even in the hypotheses, of the workers of half a century since: and in the present paper I propose to trace, in a little detail, what I believe to be a correct outline of the development of two of the more important theories of modern chemistry.*

The chemical views most in vogue before the strictly modern epoch, were founded more on considerations of the composition of compounds than on the actions of these compounds. Dumas introduced wider views by recalling the attention of chemists to the fact that in order to frame even a tolerably complete system of classification, an answer must be given to the question, 'What does this substance do?' no less than to the other question, 'Of what is this substance composed?'

* In the paper referred to, I briefly sketched the history of the development of the older doctrine of 'Equivalents' into the modern hypothesis of 'Valency.'

But if we go back to the time before Lavoisier and his associates, we find that the system then predominant in chemistry was founded almost entirely on the reactions, and but to a very small extent on the composition of chemical substances.

Chemists then busied themselves continually with studying processes of chemical change; only they contented themselves with qualitative knowledge, and hence their hypotheses were for the most part extremely vague and their facts disconnected.

John Joachim Beccher, born about 1630, seems to have been the first to weave together the scattered chemical facts and guesses into a consistent general theory, which was subsequently augmented and defined by Stahl (1660–1734).

Looking at the wonderful changes produced in substances by the action of chemical force, the question arose, what happens when a body undergoes chemical change? and as burning or combustion was perhaps the commonest of all chemical changes, the question became narrowed, and chemists eagerly sought for an answer to the query, 'What happens when a chemical substance burns?'

In those days natural phenomena were referred to the presence of 'principles' or 'essences' in the matter exhibiting the phenomena. A new principle was added to the list; and the question was supposed to be solved by saying that combustible substances are characterized by richness in *Phlogiston*, (Gr. Phlogizō = burn, or inflame), and that when they burn they lose this principle, so that the burnt substance, or calx, consists of the original substance *minus* Phlogiston.

The Phlogisteans seem to have regarded their hypothetical principle as a modified form of fire, as fire confined in a material substance; but as they gave no definition of fire, beyond saying it was one of the four elements, it was scarcely to be expected that they should define Phlogiston.

By restoring Phlogiston to the burnt substance, said the theory, the original matter is regenerated. Some substances, *e.g.*, charcoal, are especially rich in Phlogiston, and metallic calces may be converted into metals, *i.e.*, may be unburnt, by heating them with charcoal.

Thus the Phlogisteans regarded the phenomena which they studied in a purely qualitative manner: they asked only What does this or that substance do under given conditions? not being aware that a full answer to this question can only be given, when the other question—*How much* of some given effect is produced by a given quantity of this body under stated conditions? had been answered.

The introduction and use of the balance carried the day in favour of those who opposed Phlogistic views. If a substance

loses something when it burns it *must* weigh less than before burning—as a fact it weighs more—therefore it has not lost but gained something.

'Nay,' replied the Phlogistean, 'it has lost something, but the weight of this something can only be expressed by a negative quantity.'

'But a something with such properties is an absurdity,' replied the opponent, 'therefore it has no existence, and therefore your theory is utterly false.'

The anti-Phlogistean triumphed, and the principle of levity was banished from chemical science.

But the principle returned in a modified form. Lavoisier, who opposed the Beccherian theory of Phlogiston with signal success, himself propounded a theory of the constitution of solids, liquids, and gases, in which the 'subtle principle' '*caloric*' played an important part. Lavoisier regarded oxygen, as what he termed 'concrete oxygen' *plus* a something—caloric; indeed he appears to have looked on all substances in the concrete state as solids, and to have supposed that the addition of a certain quantity of caloric to these caused them to become liquids, whilst the addition of a further quantity of caloric produced gases.

Thus chemists seemed obliged to imagine a something in addition to the gross or ponderable matter of which bodies are composed, in order to account for the properties of these bodies. As science has advanced she has been able to define what this something is, at least, she has defined it more clearly than the older workers could do.

I have said as science has advanced she has defined the unknown something; but it should be remembered that that wonderful book, which contains—according to the greatest authorities—the germs of all our modern advances, was written sixty years before Lavoisier's time. Sixty years before the apparent overthrow of the theory of Phlogiston, Newton had laid the foundations of the science which was to reveal the true lineaments of that Unknown whom the Phlogisteans ignorantly worshipped.

We have learned to extend the meaning of the word *thing*—we speak of 'the power of doing work' as a measurable and definite thing—although not as matter: and we know that when a body burns it loses a certain amount of this power of doing work, or, as it is more shortly put, of energy.

As usual it is a question of words. The older workers could not define Phlogiston; we are able to define energy, and therefore, we can see clearly where they saw but darkly. Chemistry now acknowledges that the properties of a compound are not only determined by the composition of the

matter of that compound, but by the amount of energy associated with that collocation of matter. She has been able to point out many instances of compounds composed of the same matter, but possessed of different amounts of energy, and, at the same time, of very different properties. And moreover, chemistry aided by physics, has concluded that the properties of a body 'are dependent on the variations of the energy of the body, and not on its total value,' and therefore that 'it is unnecessary, even if it were possible, to form any estimate of the energy of the body in its standard state.' (I quote from that remarkable little book of the late Professor Clerk Maxwell, *Matter and Motion.*)

Whenever science made the advance from the vague conception of 'principles' and 'imponderable matter' to the definite conception of 'mass,' 'motion,' and 'energy,' she was able to recognize the truth which lurked under the cumbersome and inexact nomenclature of the Phlogistean chemists.

I have said that, as usual, the dispute between the Phlogisteans and their opponents was proved to be a question of meaning of words: as usual, also, subsequent research showed that while both were wrong, both also were right.

Composition is important, but composition is not all. The burnt body has properties differing from those of the unburnt body, because it has lost a certain amount of 'the power of doing work;' but it has a less power of doing work because it is possessed of a structure different from that which it possessed before. Composition and properties, energy and structure, are closely connected: to determine the exact relations existing between these, under stated conditions, is still the fundamental problem of chemical science.

We can define Energy: the Phlogisteans could not define Phlogiston. But in the ethereal philosophy of the future will it not be said of the present workers in science that they could not define Ether, but even spoke of it at times as 'not gross nor ponderable matter?'

The theory of Phlogiston was continued and developed in the theory of Caloric: the theory of Caloric is vastly extended, simplified, and rendered definite in the theory of Energy: and the theory of Energy seems destined to be largely extended by the Ethereal theory now in its infancy.

Mankind has until lately been content with space of three dimensions, but the bolder and more dashing spirits among the mathematicians have dared to look forward to a better world than this where they may revel in space of four dimensions. What a strange world must that be! what a fearful place for a mathematical examination, when we remember that the inhabitants thereof — if there be inhabitants — may turn

spherical hollow balls inside out without tearing or breaking them!

While we look forward to the future of science with hope, I think we ought not to look back on the former workers without respect.

But I must pass on to consider the second of the great theories which have paved the way for the doctrines of modern chemistry. The germ of the modern ideas of substitution, valency, atom-linking, &c., is, I believe, to be found in the pure dualism of Berzelius, and moreover, the influence of the dualistic ideas of that great chemist seems to me easily traceable in the essentially unitary system of modern chemistry. The chemistry of Lavoisier centred around the wonderful substance whose properties he so carefully studied. The teaching of the great founder of modern chemistry was saturated with ideas suggested by the study of oxygen. The compounds of oxygen were divided by Lavoisier into two groups, bases and acids: when these reacted chemically, a salt—that is, a body made up of base and acid—was produced. Berzelius developed these ideas until he had constructed a complete and beautiful theory, viewed in the light of which all compounds were of analogous structure. Every chemical substance was made up, according to the Swedish chemist, of two parts; these parts might themselves be composed of simpler parts, or they might be truly elementary. The two parts of a compound were respectively endowed with positive and negative electricity. When two bodies combined the positive electricity in one neutralized the negative electricity in the other; hence the phenomena of light and heat noticed in chemical combination. An element might contain an absolutely greater quantity of positive electricity than another and nevertheless belong to the electro-negative series of elements: thus sulphur and oxygen readily combine to form a substance which, when dissolved in water, yields an acid. But oxygen and sulphur are both electro-negative elements. Berzelius supposed that sulphur contained a large quantity of both electricities, the negative predominating. When this element combined with oxygen, the positive electricity of the sulphur was supposed to be neutralized by the negative electricity of the oxygen, so that the negative electricity of the sulphur was concentrated or rendered more apparent. The affinity between oxygen and silver is less than that between sulphur and oxygen, because, said Berzelius, silver contains mainly positive electricity, but a smaller quantity than is found in sulphur. The product of the union of oxygen and sulphur, *i.e.*, of oxygen with an electro-negative body, belongs to the class of acid oxides; the product

of the union of oxygen and silver, *i.e.*, of oxygen with an electro-positive element, belongs to the class of basic oxides.

If this view of the composition of oxides were granted—and a most ingenious and plausible theory it was—why should we not proceed a step further and say that an acid acts so readily upon a base, because in the first, negative electricity predominates, while the prevailing electricity in the latter compound is positive?

And in further support of this view could it not be experimentally demonstrated that when a salt, such as sulphate of sodium, is decomposed by the electric current, the soda goes to the negative pole, whilst the sulphuric acid appears at the positive pole? The experiment of decomposing a solution of sulphate of sodium was frequently performed, and the fact, that if the solution were coloured with litmus, that portion around the negative pole retained its blue colour, whilst that around the positive pole became red, was regarded as conclusive evidence of the dualistic structure of the salt operated upon.

But about the year 1834 Dumas told the chemical world that chlorine was capable of 'laying hold of the hydrogen of certain bodies and replacing it atom for atom.' If this be so, said Berzelius, the compound formed must differ essentially from that from which it is derived. Chlorine is an electro-negative element, and if it enter into a compound in place of the electro-positive hydrogen, the original compound and the new compound can present no points of analogy. The theory seemed correct, but unfortunately the chlorinated body did present very marked analogies with that from which it had been produced. Berzelius attempted many explanations, invented many new compound groups of atoms, which should be supposed to enter into the composition of the new bodies discovered by Dumas; but his electro-chemical theory was doomed. It was gradually abandoned by most chemists, and the substitutionists carried the day.

Berzelius had largely availed himself of certain facts, which showed that, in series of reactions, it was sometimes possible for a group of dissimilar atoms to remain intact, to move about, so to speak, from one compound to another without falling to pieces. Reasoning on these facts, he constructed formulæ for all compounds, which formulæ were made up of two parts, or radicles. The idea of compound radicles was thus closely associated with the dualistic theories of the Berzelian school. The new school, led by Dumas, finding dualism insufficient to explain many weighty facts, naturally waged war against the fundamental conception of compound radicles, but they were soon obliged to accept the essential truth of the theory which

they at first opposed. Liebig and Wöhler's research on oil of bitter almonds led to the discovery of a number of compounds, exhibiting many general analogies, which could best be explained by supposing the existence in each of a compound radicle, or group of atoms. When it became necessary once more to adopt the idea of compound radicles, the theory of substitution was found to be strengthened, not weakened, thereby. Many reactions were made clear by supposing that an element might be substituted by a group of elementary atoms, by a compound radicle. But in adopting the idea of compound radicles the substitutionist yet maintained that the chemical compound was a distinct whole, made up of parts he admitted, but, nevertheless, having these parts so modified and merged in one another that the resultant acted as a homogeneous compound. Thus when the new school likened the ethers to the metallic oxides, they did not mean to assert that the molecule of ether was composed of two parts, ethyl and oxygen, held together by electric bonds, and ready to part company without difficulty; nor, in asserting that ether was one substance, and not a dualistic system, did they deny the existence of a structure within the molecule of ether. They admitted the existence of a closer relationship between the atoms of carbon and hydrogen constituting the group ethyl, than between these atoms and those of oxygen, and they generalized the reactions and analogies of ether, by saying that it might be regarded as sodium oxide in which both sodium atoms had been substituted by two compound atoms of ethyl. Berzelius had himself likened the ethers to oxide of potassium, and by doing this the great apostle of dualism had paved the way for the advance of the unitary theory.

That portion of the dualistic doctrine which was embodied in the theory of compound radicles was adopted by the unitary schools, but adopted in a modified form: the effects of this modification were not long in making themselves felt.

Berzelius, in his later works, had been ready to give a dualistic formula to any compound without stopping to inquire into the facts known about that compound: he had tended to forsake the only true scientific method, and to substitute the vagaries of his fancy for the facts of nature. The new school averred that 'compound radicle' was an expression generalizing a class of facts; that the reactions of bodies were most simply explained by supposing that when acted on by chemical force the little parts of these bodies behaved as having a definite structure; and that therefore the formula of a given body might be written as containing different compound radicles under different conditions.

The fault of the old chemistry was that more attention was

paid to symmetrical formulæ than to reactions; the merit of the new consisted in bringing the student once more back to nature.

And the appeal to nature was answered and answered abundantly. The new conception of compound radicles was rich in results; from it there was developed,—first, the theory of types, and subsequently the wider theory of valency, which has led to that of atom-linking, and these in their turn have reacted on the older and more fundamental notions of the science, and have given a new meaning to such terms as 'chemical' and 'mechanical actions,' 'compounds' and 'mixtures,' &c., while, at the same time, they point the way to the chemistry of the future when we shall have gained a definite conception of the inner mechanism of the molecule, and of the laws which regulate the combinations of molecules in groups, and the decompositions of molecules with subsequent formations of new atomic systems.

Let us shortly examine these ideas. If sodium be thrown on to water caustic soda is produced, a substance made up of hydrogen, oxygen, and the *simple radicle* sodium; by another reaction a substance can be obtained, consisting of hydrogen, oxygen, and the *compound radicle* nitryl, (NO_2). These two bodies have analogous formulæ, Na OH and (NO_2) OH, they may both be regarded as derived from water, H HO, by the replacement of one-half of the hydrogen by a radicle; in one case by Na, in the other by NO_2. Again the whole of the hydrogen in water may be replaced by sodium, with production of the compound sodium oxide, Na_2O; but in many of its reactions this compound is the analogue of common ether, which is also a compound of oxygen with a (compound) radicle *Ethyl*, and has the formula $(C_2H_5)_2O$. Now these substances, Na OH, (NO_2) OH, Na_2O, and $(C_2H_5)_2$ O, both on account of the methods by which they are produced, and because of their general reactions, may be classed together as derivatives of water, or may be said to belong to the *water-type*. Similarly, other types have been instituted, and large groups of compounds have been brought into the same class as being all referable to one parent type. This step in advance is evidently an outcome of the theory of compound radicles: without that conception a system of classification by types would have been impossible.

But it was found that while such compound radicles as C_2H_5 or NO_2 were capable of replacing but one part by weight of hydrogen in water, other compound radicles, such as CO or C_2H_4, were capable of taking the place of two parts by weight of hydrogen. Comparing together these two sets of radicles, it might be said that $CO = 2\ NO_2$ or $C_2H_4 = 2\ C_2H_5$, so far as the power of combining with hydrogen was concerned. This

conception of binding power being extended to the elements, and being deepened and widened by laborious experimental researches, led to the general theory of valency, which included in itself the essential features of the older doctrine of equivalents.*

Having thus gained the conception of a definite binding power as applicable to elementary atoms or groups of atoms, it followed, as an almost necessary deduction, that the smallest parts of chemical compounds which existed as distinct chemical entities, *i.e.*, the molecules, must have a definite structure: that the parts (atoms) of the little systems must be arranged in accordance with the valencies, or binding powers, of these parts.

Hence, given the number of atoms in a molecule, and the valency of each atom, it became possible to calculate the number of different arrangements of these atoms which could be produced; and careful experiment has often succeeded in preparing all the different, theoretically possible, compounds. The difference of properties of such compounds, *i.e.*, of compounds the molecules of which are constituted of the same number of the same atoms, but differently arranged, is attempted to be indicated in the 'structural' or 'rational' formulæ of modern chemistry.

Berzelius spoke of compounds composed of parts held together by mysterious bonds: the idea survives in these structural formulæ of to-day, only we are now able to define what we mean by the smallest part of a compound having a chemical existence, and we have gained certain generalizations which enable us to trace with some degree of accuracy the relationships which exist between the inner parts of these smallest chemical wholes. We appear to be now fairly embarked in the prosecution of molecular dissection, and our chief guide is the theory of valency, itself a development of the dualistic chemistry.

Each elementary atom, I have said, seems to have the power of directly binding to itself a maximum number of other atoms; but it would further appear as if the groups of atoms thus produced had also a certain binding power, but this more indefinite than the atomic binding power, and very variable under different physical conditions. This atomic binding power appears to have a fixed maximum value, but not always to reach the maximum. What is the exact way in which the binding power or valency of the elementary atoms is influenced by definite changes in physical conditions? This is one of the most important unsolved problems of general chemistry.

Then, again, granting the existence of an inner structure to

* See former paper, Jan. 1878.

the molecule, granting that groups of atoms do exist in the molecular building, does the fact that in a certain reaction certain atoms are withdrawn as a group, prove that these existed in the form of the same group in the original molecule? In other words, do our structural formulæ express the relative collocation of atoms within the molecule while the molecule is unacted on by extraneous force, or do they merely roughly represent the condition of things when the molecule is in a state of strain, because of the stress between its parts and those of another molecule, or molecules, brought within its sphere of action? Here is another question which can only be answered after much experimental evidence has been accumulated. Now these questions, I make bold to say, are the direct outcome of the dualism of Berzelius, modified by the unitary chemistry of Dumas and his followers.

If we glance back on the development of the two theories, the course of which I have endeavoured to outline, we find that both began with a purely qualitative study of reactions, but that it was only when to this had been added the careful use of weights and measures, that any solid advance became possible. Further, we find that the older theory was founded chiefly on a study of reactions, whilst that which was broached after the time of Lavoisier was founded most largely on a study of composition. With the Phlogisteans *function* was of paramount importance; with the Dualists *composition* was all. The modern theories, which have been developed from these, have attempted, with varying success, to combine both considerations. And if we examine the latest advances of theoretical chemistry we still find it at work on these two lines of advance. The composition of chemical compounds is studied by the majority of chemists; but the general laws of action of chemical force itself have of late received most important elucidation.

Again, if we look to the 'lines of advance along which dynamical science is working its way to undermine, at least, the outworks of chemistry,' we can distinguish two, essentially the same, lines as were used by the two classes, whose theories I have dealt with in this paper. 'One is conducted by the help of the hypothesis that bodies consist of molecules in motion, and it seeks to determine the structure of the molecules and the nature of their motion from the phenomena of portions of matter of sensible size. The other line of advance, that of Thermodynamics, makes no hypothesis about the ultimate structure of bodies, but deduces relations among observed phenomena by means of two general principles, the conservation of energy, and its tendency towards diffusion.' (Clerk Maxwell. *South Kensington Science Conferences*, 1876, p. 145.)

I have thus sought to substantiate the claim of the new chemistry to be a development of the old. I believe that if this claim is granted, the conclusion to be drawn must be, not that the old is better, but that to return to that which is admittedly an early stage of development would be to misread all the teachings even of the old chemistry itself.

One general lesson may surely be deduced from what has been said, and that is, the continuity of science. Science proceeds by gradual developments, each dependent on that which went before. She may frequently be obliged to review her past progress, and even, in the light of freshly acquired knowledge, to alter what once appeared to be well-established generalizations. But if any generalization of science be founded on experimentally authenticated facts, it always survives, although not necessarily in its commonly accepted form. The Phlogistic theory was absorbed in, not entirely contradicted by, the Caloric theory of Lavoisier, which in its turn was merged in the fuller and more definite modern doctrine of Energy. The dualism of the Swedish school appeared to be absolutely overthrown by the followers of Dumas, but the theory has survived, and, modified to suit the conditions of its environment, forms one of the groundworks of the chemistry of to-day.

In examining the progress of Science, we see that she is not afraid to retrace her steps, and that she is able to retain and develop all that is probably true, whilst rejecting all that is proved to be false; and when we learn that she does this, can we hesitate to find in her history the 'promise and potency' of a mighty future?

CAIUS COLLEGE, CAMBRIDGE;
February, 1880.

THE CLASSIFICATION OF THE TERTIARY DEPOSITS.

By Prof. JOHN W. JUDD, F.R.S., Sec. G.S., &c.

THERE are probably few achievements of genius which will bear comparison with William Smith's famous discovery of the true order of succession among the British stratified rocks. Whether we regard the state of knowledge upon the subject at the time when he commenced his labours, or consider how few are the changes which have been introduced into his scheme of classification by the work of subsequent observers, we shall be equally convinced of the justice of his claim to rank as 'the Father of English Geology.'

In his original table, drawn up in 1799, William Smith divided the British strata into twenty-three groups; the highest being the Chalk and the lowest the Coal. And though in subsequent tables he introduced both older and younger formations than these, it is evident that the series of deposits of which the order of succession was perfectly made out by him, embraced only the strata from the Carboniferous to the Cretaceous rocks inclusive. Thus, in 1815, Smith grouped all beds below the Mountain Limestone as Red Rhab, Dunstone, Killas, and Slate, while those above the Upper Chalk are classed in upward succession, as 1, Sand; 2, Crag; 3, Sand; and 4, London Clay.

It remained, therefore, for William Smith's successors to complete his work, by classifying, on the principles which he had laid down, the strata which are older than the Carboniferous and those which are younger than the Cretaceous. With respect to the former, the labours of Sedgwick, Murchison, and Lonsdale, left little to be desired, and the result of these labours was the establishment of the Cambrian, Silurian, and Devonian systems. The distinguished founders of the Palæozoic classification followed William Smith's method of determining the order of succession among strata, which con-

sisted in tracing the beds, as far as possible, by their outcrops at the surface, and determining their superposition in sections, falling back where these methods failed them on 'the identification of the strata by their organic remains.' In the system of nomenclature which they adopted, however, the discoverers of the Palæozoic succession introduced a noteworthy improvement on the method of their great teacher. Instead of adopting names based on the accidental characters of the strata, such as 'Lias,' 'Cornbrash,' and 'Gault,' they employed local names, calling each larger or smaller group of beds after the place in which it was found most typically developed, as the 'Aymestry Limestone,' the 'Bala Formation,' and the 'Devonian System.' This method has been found so convenient in practice, that similar local names have since been given by geologists to many of the divisions of the Secondary strata which had been established by William Smith.

It may, at first sight, appear that in classifying the strata above the Chalk, geologists ought to have followed the same methods and adopted the same principles of notation as had proved so successful in dealing with the older deposits; and it must be admitted that such a course would have produced a uniformity and simplicity in our scheme of geological nomenclature which does not now exist. But a little consideration will convince us that these principles of classification and nomenclature were not abandoned in the case of the Tertiary strata without good cause.

The Tertiary deposits differ from those of Secondary and Palæozoic age in several very important particulars. While the older strata are always of marine and estuarine origin, the Tertiaries include lacustrine and terrestrial beds, often of great thickness. The cause of this difference is found in the fact that, while in the case of the Tertiary deposits, the sea-beds have, perhaps, only once been elevated into dry land since the beds were accumulated, in the case of the older formations they have been subjected to many successive subsidences and elevations and from the denudation which took place during these movements only the deeper-water and more widely-spread sediments have been able to escape. It has been well remarked by Mr. Darwin that 'nearly all our ancient formations, which are throughout the greater part of their thickness *rich in fossils*, have been formed during subsidence;' and it is well known to all geologists that as we go backwards in the geological series we soon find that nearly all traces of terrestrial accumulations disappear; we next lose all relics of fluviatile and lacustrine deposits; then we find littoral marine deposits becoming rarer and rarer; till at last, among the most ancient strata, we seldom find any but such as must have been laid down in deep water,

and are consequently spread over wide areas. Now while the methods of William Smith proved so successful when applied to the uniform and widely-spread deposits of older date, it was soon found exceedingly difficult, if not impossible, to trace the order of superposition, and to represent on maps the outcrops of the inconstant patches of sand, clay, and shell-banks, which often make up the Tertiary formations.

It is to the late Sir Charles Lyell that geologists are indebted for the suggestion of a method whereby the local and inconstant patches of Tertiary strata, which are scattered all over Europe, might be brought into comparison with one another and arranged in groups according to their relative ages. The discovery of this method of classifying the Tertiary strata was an achievement only second in importance to William Smith's determination of the order of succession among the Secondary formations, and would, of itself, serve to place Lyell's name in the foremost rank among the founders of the science of Geology; even apart from the claim which he derives from his masterly exposition of the philosophy of the science contained in the immortal *Principles of Geology*.

The study of the Tertiary formations, as of so many other branches of Geology, was commenced in Italy. After the great discussions as to the true nature of fossils, which occupied the minds of the thinkers of that country during the seventeenth century, had been brought to an end by the general acceptance of the doctrine that fossils are not accidental simulacra, but actual relics of once living beings, many able naturalists turned their attention to the study of the rich series of fossils contained in the Subapennine strata. Soldani, Testa, Fortis, Cortesi, Spallanzani, and other Italian naturalists, all recognized the important fact, that while some of the fossils found in the Subapennine strata are referable to forms still living in the Mediterranean, others are now only found in tropical seas, while others again have never been discovered among recent forms and are presumably extinct.

At the commencement of the present century, the illustrious Italian geologist, Brocchi, had made a very extensive collection of the Subapennine fossils, and by a comparison of these with recent shells, he had arrived at the conclusion that more than one half of the Subapennine forms are still living in the Mediterranean, or in other seas, chiefly those of hotter climates. The labours of Brocchi were supplemented by those of Bonelli, Guidotti, and Costa, who added greatly to our knowledge of the several Subapennine faunas.

While this important work was being carried on in Italy, naturalists were not idle in other parts of Europe. Cuvier and Brongniart described the succession of strata in the Paris Basin,

and Lamarck discussed the characters of the invertebrate fauna found therein. In a catalogue of five hundred species of shells found in the Paris Basin, Lamarck showed that only twenty could be identified with living species.

In England, Brander, as early as the year 1766, had described the beautiful fossil shells found at Barton Cliff, in Hampshire, though without discussing their relation to living forms. In 1811 Parkinson described the Crag beds of Suffolk as overlying the London Clay, and as containing numerous fossil shells, of which a considerable proportion could be identified with species now inhabiting the neighbouring seas. About the same time Webster was engaged in studying the Tertiary strata of the Isle of Wight and Hampshire, and in 1813 he established the general parallelism of these beds with those described by Cuvier and Brongniart in the Paris Basin.

In 1820 M. Constant Prévost described the strata of the Vienna Basin, the study of which had occupied his attention during four years, and announced, as his conclusion concerning their age, that they are either younger than the beds of the Paris Basin or equivalent to the upper portion of them.

In the year 1825, De Basterot published an account of a large Tertiary deposit which he had discovered in the basin of the Gironde and the district of the Landes in the south-west of France, and he described more than three hundred species of shells as occurring there. These, as he proved, differ, for the most part, both from the shells of the Subapennine beds, and from those of the Paris Basin. A little later, M. Desnoyers showed that beds with a similar fauna to that collected by De Basterot about Bordeaux and Dax, are found at Touraine, in the valley of the Loire, while Bonelli recognized the same fauna in the hill of the Superga, near Turin.

At the time when Lyell took up the investigation of the subject, the state of the problem was as follows. Three distinct sets of Tertiary strata had been discovered. *First*, the Subapennine beds of Italy and the Crags of England, in which a majority of the shells were found to belong to living species. *Secondly*, the series of strata of the London, Paris, and Hampshire Basins, in which only a very small minority of the shells belong to living species. *Thirdly*, the strata of the Landes, and the Faluns of Touraine (with which those of the Vienna Basin and of the Superga in Piedmont were identified), in all of which the fossils were in great part distinct alike from those of the Subapennine beds, on the one hand, and from those of the Paris and London strata on the other.

One other very important step had been taken by M. Desnoyers, who showed that in the valley of the Loire the beds of Touraine distinctly overlie others containing the same fossils as

those of the Paris Basin. The Italian geologists had also proved that the beds of the Superga underlie ordinary Subapennine strata. Hence it became evident that the formations of the Faluns of south-western France, of the Superga, and of the Vienna Basin, are of younger age than those of the Paris, London, and Hampshire Basin, and of older date than those of the Subapennine hills of Italy and the Crags of England.

It was Lyell who first recognized the remarkable significance which attaches to the fact that the faunas of these series of strata are found to contain gradually increasing numbers of living species as we pass upwards in the series. Brocchi had remarked upon the different results arrived at by Lamarck and himself in comparing the Paris and Subapennine shells respectively with those which are found living in the seas of the present day. But he suggested that the fact might be explained by the difference which undoubtedly exists between the testacea of the Mediterranean and those of the Atlantic Ocean. In 1831, when he completed the second volume of the *Principles of Geology*, Lyell had carefully considered the question of the gradual extinction of old forms and their replacement by new ones, although, as is well known, he felt himself unable to accept any of the theories, which, up to that time, had been suggested for the explanation of the latter class of facts.

All who had the good fortune to know Lyell will recognise it as eminently characteristic of his earnest and truth-loving nature that he delayed the completion of his *Principles of Geology* until he had been able to investigate personally the points at issue, so as to make his descriptions and reasonings as clear as was possible under the circumstances. In the early part of 1828 he had already begun to feel the difficulties which beset the classification of the Tertiary deposits, and he spent the summer in examining the various districts of France and Northern Italy; and in the autumn and winter of the same year we find him busy with the corresponding strata in the south of Italy and in Sicily. In 1829, on his way back to England, he revisited Piedmont and some of the French localities, and the summer of that year was devoted to the study of the English Crag beds. In the summer of 1830 the Tertiary deposits of Catalonia, the Pyrenees, and the south of France, were visited by Lyell; and on his return in the autumn, six weeks were spent in studying the great collections of Tertiary fossils brought together by M. Deshayes in Paris.

During these journeys and studies Lyell became convinced that the determination of the proportion of living forms in the marine testaceous fauna of any deposit would enable us to refer it approximately to its position in the geological series; and in

attempting to put this conclusion to the test, he availed himself of the valuable assistance which so able a conchologist as M. Deshayes was able to afford him. Lyell was so impressed by the distinctness of the Sicilian strata, which contain scarcely any but living forms, from those of the Subapennine hills, in which only about one half of the species can be identified with living ones, that he had already determined, before discussing the matter with Deshayes, to divide the Tertiary strata into *four* groups. M. Deshayes had, however, classed his shells in conformity with the facts which had been already made out by geologists into the *three* groups which we have already indicated.

In the choice of names for the great divisions of the Tertiary series, which he was thus led to establish, Lyell determined to indicate the methods by which his results had been arrived at. After consultation with Dr. Whewell, he proposed to call the oldest strata, in which Deshayes' tables showed only 42 forms out of 1238 to be still living (a proportion of three and a half per cent), the Eocene (ἠως, dawn, and καινος, recent), as the beds may be regarded as exhibiting the dawn of the existing fauna of our seas. In the second group of Tertiary strata Deshayes' tables showed 1021 species, of which 176 only were recent forms; while in the third group it was found that more than one half the forms could be identified as still living; hence Lyell called the second group Miocene (μειων, minor, and καινος, recent), as possessing a minority of living forms, and the third, the Pliocene (πλειων, major, and καινος, recent), as yielding a majority of living species. In his original classification Lyell was contented to employ these three terms only, dividing the Pliocene into Older and Newer. In 1839 he erected the Newer Pliocene into a distinct system, in accordance with his original views, giving it the name of Pleistocene (πλειστος, most, and καινος, recent); but as Edward Forbes and other writers employed this term in a sense quite distinct from that proposed by its author, and as synonymous with Post-Pliocene, its use, as indicating a division of the Tertiaries, was formally abandoned by Lyell.

This nomenclature of the Tertiary strata which was proposed by Lyell has been frequently criticized, and is certainly open to the objection that it differs in its principles from that employed in the case of the older rocks. Hence terms have been proposed by various authors as synonymous with Lyell's names, which, like the names of the older rocks, indicate their characteristic features or are derived from the localities in which they are best developed. Thus some geologists prefer to call the Eocene the Nummulitic system, the Miocene the Falunian system, and the Pliocene the Subapennine system. But, on the other

hand, it must be admitted that the Lyellian nomenclature has now acquired such general, and indeed almost universal, currency, that a change of names, which has no other object than the attainment of an impossible uniformity, is greatly to be deprecated. And there is, moreover, much to be said in favour of Lyell's names, not only on the ground of priority, but from the fact that they indicate the principles on which Lyell's classification is based, and serve as a monument—the best possible monument, indeed—of the great advance in systematic geology which resulted from his labours.

The value of the Lyellian method of classification is illustrated by the fact that in the third volume of the *Principles* its author was able to bring into more or less exact correlation the numerous and widely scattered Tertiary deposits which had been detected in various parts of the Continent. Since 1833, when this work appeared, Lyell's broad general outlines of the succession of events during the Tertiary epoch have been filled up in great detail by the labours of Prestwich, Constant Prévost, Hébert, Dumont, Sandberger, Beyrich, Hörnes, Fuchs, Heer, Carl Meyer, and other investigators; and in the application of Lyell's method, its capabilities, and also its imperfections and shortcomings, have been brought to light by the test of practical experience. It will be instructive to notice the respects in which the Lyellian method, after being tried, has been found wanting.

First among these, we may notice the circumstance that, as the acquaintance of geologists with the Tertiary formations has become more and more extended, deposits have come under their notice which it has been found difficult to refer to either of Lyell's divisions, and which prove to constitute transitional formations linking two of them together. Thus the geologists of South Germany have shown that in the Vienna Basin the Miocene strata graduate so imperceptibly into the Pliocene, that they have been led to unite these two divisions, and call them the 'Neogene.' But this class of objection cannot be regarded as especially applying to Lyell's classification, for it is certain, from the very nature of the case, that whatever grouping of strata we may choose to adopt, certain deposits must be found which refuse to accommodate themselves to the artificial system.

A more serious objection to Lyell's classification is based on the fact that at the period when he devised it, geologists were not acquainted with all the great representative faunas of the Tertiary epoch, and that since his time very important deposits have been discovered which cannot be referred to either of Lyell's groups. The great brown-coal deposits of Northern Germany, which are now known to be of the age of the English Barton Clay, the highest member of the Eocene, are reached

by shafts, which penetrate a great thickness of drift-sand, gravel, and clay. Immediately overlying the brown-coal, beds containing marine shells were found at many points by the German geologists; and these fossils, when carefully studied, were found to differ so greatly from those of the Eocene on the one hand, and the Miocene on the other, that it was difficult to refer the beds containing them to either of Lyell's divisions.

At the same time, the researches of Nyst and Dumont in Belgium, and of Sandberger in the Mayence Basin, made geologists acquainted with other deposits containing a fauna similar to that found in the North German beds, and, like it, distinct from the faunas of both the Miocene and the Eocene.

Somewhat later, the execution of certain new railway works near Paris enabled MM. Hébert and Raulin to make a careful study of the fauna of the Fontainebleau Sands, which had hitherto remained almost unknown. And in these beds were found assemblages of fossils agreeing very closely with those of the North German and Belgian localities, but differing, like them, from the faunas both of the Eocene and Miocene deposits.

Lastly, the construction of a branch of the South-Western Railway through the New Forest in Hampshire led to the exposure, in a railway-cutting near Brockenhurst, of marine beds containing many mollusca and corals, which were collected with untiring industry by the late Mr. F. Edwards. Similar fossils had previously been found at Lyndhurst and at some other points, both in the New Forest and in the Isle of Wight, but those obtained in the Brockenhurst railway-cutting were so numerous and well preserved as to awaken general attention and interest in the subject. An examination of the Brockenhurst mollusca by Herr von Koenen proved conclusively that they agree most closely with the fossils of the North-German clays and sands, with those of the Fontainebleau Sands, and with those of the equivalent strata in Belgium and the Mayence Basin. Dr. Duncan's examination of the Brockenhurst corals led him to precisely the same conclusion, which was arrived at by him quite independently of Von Koenen's researches.

It thus came to be a recognized fact that in North Germany, in the Mayence Basin, in Belgium, and in the Paris and Hampshire Basins, there exist strata which at some points are seen to be superimposed on Eocene strata, and at others are found underlying the Miocene, and that the fauna of these beds is very distinct both from that of the Eocene and from that of the Miocene.

In the year 1854 Professor Beyrich proposed that to this new geological horizon, which had come to be recognized as existing between the Eocene and the Miocene, the name of Oligocene (from ὀλιγος, few, καινος, recent) should be given, the

name being formed on the same principle as was adopted by Lyell in coining his terms. Exception has been taken to this name of Professor Beyrich's, but its very general adoption may be taken to prove that it supplied a want which was beginning to be very generally felt. It is true that Lyell did not adopt this new term, but it may be safely asserted that, had he lived to witness the recent advances in our knowledge of the deposits of this period, he would not have hesitated to accept the evidences in favour of its employment. Not only in Germany, Belgium, France, and England, but in the Alps and Eastern Europe, have beds of great thickness and importance, which must be referred to this division of the geological series, been discovered, and even in North America it has been shown that it is convenient to employ the term Oligocene to designate a division of the Tertiary series.

It will be interesting to take a brief survey of the great series of deposits which recent researches have led geologists to refer to the Oligocene period; and in doing so we shall notice the evidence which they afford of the distribution of the land and sea, and the general physical features of Europe at the time of their deposition.

It is in Eastern Europe that the Oligocene strata acquire their greatest normal thickness and development. In Transylvania and Hungary marine and brackish-water strata of this age are found, attaining a thickness of betwen 2000 and 3000 feet. Dr. Anton Koch, of Klausenberg, has described the succession of the Oligocene strata in the neighbourhood of that town, where, resting upon beds containing the fauna of the Barton Clay, we find a thick series of deposits which can be referred to the Upper, Middle, and Lower Oligocene respectively. I have myself had the opportunity of studying the succession of these Transylvanian beds, and of noticing how the series of fossiliferous beds in that area can be paralleled with those of northern and western Europe. A similar succession has been made out in the Bihar Mountains and in other parts of Transylvania. As we proceed southward, however, towards the Turkish frontier, we find brackish-water and terrestrial conditions prevailing over the marine. This is well illustrated in the Tsil Valley, in the south of Transylvania, where at Petroseny a coal-bed, nearly ninety feet in thickness, is being worked in open pits and by means of adits. This great coal-seam and others of lesser thickness are intercalated in a series of strata, which contain a characteristic Oligocene fauna, the strata in question having been preserved from denudation through being thrown into a synclinal fold.

In the country round Buda-Pest the Oligocene strata have been carefully studied by Dr. Szabó, and their fossils have been

described by Dr. Von Hantken. A deep well-boring near the city has fully confirmed the conclusions which had been arrived at as to the great thickness (more than 2000 feet) of the marine Oligocene strata in this area. Underlying the great masses of Miocene lava that cover so large a portion of Hungary there are found at many points marine strata, which by their fossils must be referred to the Oligocene. These beds, as developed near Vissegrad, have been well described by Dr. Koch.

When we proceed westward, into the Vienna Basin, we find that the Middle and Lower Oligocene strata are altogether wanting in that area, and only the Upper Oligocene is represented by the 'Aquitanische Stufe.' In passing southwards, into Styria, Croatia, and Sclavonia, these Upper Oligocene strata are found to consist of brackish-water and terrestrial deposits, just as is the case farther west in Transylvania.

In Northern Germany, throughout the whole of the drift-covered districts, stretching from Warsaw to Hamburg, patches of Oligocene strata are here and there found rising above the superficial covering of the country, and the same deposits are constantly met with in wells and borings. These strata contain marine fossils, sometimes of littoral, at others of a deeper-water character. It is in these beds that the German geologists have discovered such a rich molluscan and coral fauna. The most abundant stores of fossils have been obtained during the sinking of pits to the brown-coal beds (which are of the age of the Barton Clay), the shafts often passing through richly fossiliferous clays and sands before reaching the lignite beds.

As is the case in Hungary with the Oligocene strata, so we find with the marine beds of that age in Northern Germany that when traced southwards they graduate into estuarine, fresh-water, and terrestrial formations. Opening into the great North German Oligocene sea we find clear evidences of the existence of four or five great deltas at this period. The most easterly of these now forms the country of Lower Silesia; next we have the delta of the Saxon district; thirdly, we find the delta of the Lower Rhine, of which the Mayence Basin may be regarded as a part; and fourthly, the estuarine deposits of Oligocene age in the Netherlands, and in the Paris, and the Hampshire Basins, which are all very closely connected with one another, and may have formed parts of several more or less united deltas. In the North German deltas we find beds of brown coal (distinct from and of younger date than the great Upper Eocene brown-coal formation), which alternate with sands and clays containing fresh-water or brackish-water fossils. Farther westward, we have only thin lignite deposits, the strata consisting of fresh-water limestone, clays and sands, with occasional marine beds intercalated among them.

Now while these marine and estuarine strata were being accumulated in Hungary, Northern Germany, the Netherlands, Northern France, and the British Islands, a number of great lakes existed upon the land of the Oligocene period. These were gradually filled up, and obliterated by sedimentary deposits. Owing to the fortunate circumstance that, immediately after the close of the Oligocene period, a grand outburst of the volcanic forces took place over very wide areas, these fresh-water deposits have been covered up and protected to some extent from destruction by denudation. It is thus that we find preserved for our study the interesting lacustrine deposits of the Limagne, of Montbrison, of the Haute Loire, of Menat, and of many smaller lakes in the Auvergne, and of the lake-basins of Teplitz, Falkenau, and Eger, in Bohemia. All of these lakes, with many others, of which all traces must have been removed by denudation, seem to have been in existence during the Oligocene period.

If we now turn our attention to the districts immediately adjoining the great Alpine chains, we find the Oligocene formation represented by masses of strata of enormous thickness; clays, sands, and conglomerates, accumulated sometimes in fresh-water lakes, at others in gulfs connected with the ocean. The Oligocene deposits of the Alpine district are estimated by the Swiss geologists as attaining a thickness of no less than from 10,000 to 12,000 feet, and present a marked contrast in their physical character to the marine, estuarine, and lacustrine beds of the same age in other parts of Europe. But when we come to study the fossils of these Alpine formations, the parallelism of their several members with the divisions of the Upper, and Middle, and Lower Oligocene of Northern Europe becomes strikingly apparent. In the adjoining table an endeavour has been made to illustrate the correlation of the Oligocene and underlying deposits as exhibited in different parts of Western Europe (see p. 133).

It is evident that the great and general subsidence which took place in the European area during the Eocene or Nummulitic period had already come to a close, and had been succeeded by general elevatory movements before the commencement of the Oligocene. A great part of what is now Central Europe had become dry land, while an open sea stretched to the north and north-east of it. The Oligocene deposits were accumulated along the shores, and in the deeper waters of this sea, and in the estuaries of the great rivers which flowed into it from the south. At the same time great numbers of lakes existed on the surface of this Oligocene land, and into these were washed and thus preserved for our study many of the land animals and plants of the period. Before the close of

CORRELATION OF THE LOWER TERTIARY STRATA OF WESTERN EUROPE.

		London Basin.	Hampshire Basin.	Paris Basin.	Netherlands.	Northern Germany.	Switzerland.
Oligocene.	Upper.			Freshwater Limestone of Beauce.	Beds of Maestricht.	Fossiliferous Strata of Osnabrück, Cassel, &c. Brown Coal of Rhenish Prussia.	Aquitanian Stage and Lower Brown-coal Formation.
	Middle.		Hempstead Series. Bembridge Group.	Fontainebleau Sandstone.	'Rupélien.' 'Tongrien Superior.' (Kleyn Spawen Beds.)	Septarian Clay. Marine Sand of Mayence Basin. Brown-coal Formation.	Upper Marine Molasse (Tongrian).
	Lower.		Brockenhurst Series. Headon Group.	Gypsum of Montmartre. Cerithium concavum. Beds of Mortefontaine, &c.	'Tongrien Inferior.'	Fossiliferous Beds of Egeln, Magdeburg, &c.	Lower Marine Molasse (Ligurian).
Eocene.	Upper.	Bagshot Series.	Barton Clay. Bracklesham Series and Bournemouth Beds.	Sands of Beauchamp. 'Calcaire Grossier.'	'Laekenien.' 'Bruxellien.'	Great Brown-coal Formation.	Nummulitic Strata and Flysch.
	Middle.	London Clay.	Bognor Beds.	Sands of Soissons.	'Ypresien.'	...	Lower Eocene Sandstone.
	Lower.	Oldhaven Beds. Woolwich and Reading Beds. Thanet Sand.	Plastic Clay.	Plastic Clay. Strata of Meudon, Rilly, and Nemours.	'Landénien.' 'Heersien.' Limestone of Mons.		

the Oligocene period we have proofs of the commencement of that series of volcanic outbursts which attained their climax in the succeeding period of the Miocene. But along the great Alpine axis movements on a grander scale were taking place, which resulted in the formation of vast lakes, in which great thicknesses of strata were accumulated; these lakes being sometimes, by the action of the subterranean forces, placed in communication with the open ocean. At the end of the period the elevatory forces so far prevailed over those producing subsidence, that the whole area was converted into a great continent, which remained above water during the Miocene but was to some extent submerged in the Pliocene and re-elevated in the recent period. Such appears to be the succession of changes in the physical geography of this part of the Earth's surface during the several Tertiary epochs.

It is a very interesting circumstance, as I have pointed out in a memoir recently laid before the Geological Society of London, that we have in the Hampshire Basin very beautiful and interesting representatives of at least the Middle and Lower divisions of the Oligocene system. Owing to an unfortunate error in determining the order of succession of these beds, their thickness has been hitherto greatly under-estimated, and they have been grouped with the Eocene by some authors and divided between the Eocene and the Miocene systems by others. No fact, however, can be more certain than that those fluvio-marine strata of the Isle of Wight and the New Forest are the representatives of the great Oligocene system of the Continent. The new classification which is now proposed for them is as follows:—

Upper Oligocene.	Wanting in the British Islands.
Middle Oligocene.	Hempstead Series (marine and estuarine), 100 feet. Bembridge Group (estuarine), 300 feet.
Lower Oligocene.	Brockenhurst Series (marine), 25 to 100 feet. Headon Group (estuarine), 400 feet.

Whether we study the marine mollusca, the fresh-water and terrestrial testacea, the reptilian and mammalian fauna, or the terrestrial flora of the period, we find the most convincing proofs that these strata of the Hampshire Basin are the exact equivalents of that great system of strata which has received the name of the Oligocene upon the Continent, which, as we have seen, attains to such enormous thickness and importance in some areas, and which everywhere is so well characterized by the distinct assemblage of fossils which it contains.

It is a remarkable circumstance that nearly all the great

capitals of Europe—London, Paris, Brussels, Vienna, Berlin, among the number—stand upon deposits of Tertiary age. This circumstance has doubtless powerfully contributed to the great amount of attention which has been devoted to the study of the order of succession, and the collection and comparison of the fossils of these deposits. We have already referred to the causes that prevented the earlier recognition of the importance of the Oligocene deposits, which occur but are not well exposed in the immediate vicinity of the North German capital. The Hampshire Basin has not the distinction of constituting the site of a great city, and its beds have not been so diligently explored as have those of the London and Paris Basins. The labours of Webster, Prestwich, and Forbes have, however, done much towards making clear the order of succession of the strata of the Hampshire Basin. Others, like the late Mr. Frederick Edwards and Mr. Searles Wood, have devoted themselves to the collection of the fossils of the district. It is to be hoped that the valuable fossils from the Oligocene strata of the Hampshire Basin which are contained in the British, the Woodwardian, and other Museums, a very large proportion of which still remain undescribed, may before long be made generally known to science by means of description and figures; for it is certain that a more accurate acquaintance with these Hampshire strata and their fossils will enable geologists to make valuable improvements in the classification and correlation of the lower and middle Tertiary deposits in various parts of Europe.

ARTIFICIAL DIAMONDS.

By F. W. RUDLER, F.G.S.

WHY does Science smile approvingly on the modern chemist in his efforts to produce the diamond, and yet frown upon the old alchemical notion of producing gold? If the one substance can be prepared by art, why not the other?

Everyone knows that these two bodies are the most highly valued of all natural products, and for that reason it was long suspected that some occult kinship must of necessity exist between them. Thus Pliny, speaking of the diamond, says, 'It seemeth that it should grow nowhere but in gold.' Much as the ancients prized gold, they prized this gem—the invincible *adamas*—still more. The earliest mention of the true diamond, according to the Rev. C. W. King, is by the poet Manilius, who describes it as *pretiosior auro*. 'The Diamant,' says Pliny, to quote Dr. Holland's quaint translation, 'carrieth the greatest price, not only among precious stones, but also above all things else in the world: neither was it knowne for a long time what a Diamant was, unlesse it were by some kings and princes, and those but very few.' But since those early days science has grown wondrously familiar with the diamond, and has even been bold enough to attempt its fabrication. The chemist has, in fact, outrun the alchemist: the one sought merely to make the precious metal, but the other seeks to make the yet more precious gem. Nevertheless, we treat the alchemist with ridicule, while we watch the diamond-making chemist with the keenest interest!

The truth is, that the value of the diamond, unlike the value of the gold, lies not in the *matter* of which it is composed, but only in the peculiar *form* in which that matter exists. In attempting the preparation of a diamond, we are not, therefore, striving after the impossible; we are not seeking either to create matter or to transmute one elementary species of matter

into another; all that we attempt is, to bring the given kind of matter into such a physical condition that it shall possess the set of properties which we so highly prize in the diamond.

About a century ago the chemical composition of the diamond was first carefully determined, and a fresh light was then cast upon the gem. From the day when it was ascertained that the diamond consisted only of carbon, its artificial preparation came within the range of possibility. The old notions of its kinship were entirely changed, and it was unexpectedly found that such vulgar substances as blacklead and charcoal could claim close relationship with the costly gem. Pliny ridicules the idea that the diamond could be found, as Metrodorus Scepsius had affirmed, in a locality 'wherein amber is engendered;' and the old philosopher does not hesitate to say of this authority, 'howbeit no man doubteth that he lieth stoutly.' But, after all, this notion of the relation of the diamond with amber is more sound, from a chemical point of view, than Pliny's own notion that diamonds 'breed not but in mines of gold.'

Knowing the chemical composition of the diamond, the mystery of its formation resolves itself into this problem: How to crystallize a given piece of carbon in the special forms which the gem possesses, and with the accompanying transparency, lustre, and hardness? Difficult as the solution may seem, men of science have long believed it to be practicable. 'We are so sanguine about this matter,' said Dr. Percy, when lecturing on chemical geology in 1864, 'that we cannot refrain from believing that one day or other the thing must be done. *It assuredly will be done.* We have apparently been very near it from time to time, but have never yet reached it.' These prophetic utterances have recently received a most unexpected fulfilment, which it is the purpose of this article to chronicle.

About three months ago, Mr. James Mactear, of the St. Rollox Chemical Works at Glasgow, created considerable excitement by announcing that he had succeeded in producing a crystallized form of carbon, comparable, if not identical, with diamond. It is acknowledged that this gentleman brought extensive chemical knowledge to bear upon the subject, and that he struck out a most promising line of research. Nevertheless, his announcement was confessedly premature; and it remains doubtful whether anything that can fairly be called diamond was ever produced in his researches. At any rate, the small crystalline particles which were at first taken to be diamonds, gave a most unsatisfactory account of themselves when subjected to Professor Maskelyne's searching examination,

and they utterly collapsed under the chemical scrutiny of Dr. Flight.

It has been well said with reference to other subjects that 'the failures of the past prepare for the triumphs of the future.' Nor is this saying inapplicable to our would-be diamond manufacturers. Scarcely had Mr. Mactear's investigations faded from the public mind, when Mr. A. H. Allen, of Sheffield, put in a claim on behalf of Dr. R. S. Marsden; and before this second process is revealed, Mr. J. Ballantine Hannay, a young Glasgow chemist, steps forward and actually places in our hand an artificial diamond!

For some time past Mr. Hannay has been engaged in a most interesting series of researches which have unexpectedly led up to the present discovery. To appreciate these researches it is necessary to turn to a subject which appears, at first sight, to have no bearing whatever upon the artificial production of the diamond.

More than half a century ago, Cagniard de la Tour made some remarkable experiments to determine the effect of heat upon liquids closely sealed in strong tubes. This inquiry was afterwards followed up by Dr. Andrews, of Belfast. He showed, for example, that carbonic acid gas above a certain temperature cannot be liquefied by means of pressure; but the gas, if compressed, assumes a condition which is neither that of a liquid nor that of a gas. Let the temperature be lowered, and it becomes a true liquid. Let the pressure be lowered, and it becomes a true gas. It was found that the two physical states of liquidity and gaseity pass by insensible transition one into the other; the continuity between the two conditions being perfect. That particular temperature, above which pressure does not produce liquefaction, is termed the *critical point.*

Reverting to the experiments of Cagniard de la Tour and Andrews, in which liquids were heated in closed tubes, let us suppose a solid to be dissolved in the liquid, and the solution to be then raised beyond its critical point. What will occur? The liquid will pass into the gaseous condition; but what will become of the solid? This is the question which Mr. Hannay, working in conjunction with Mr. Hogarth, sought to answer. At first sight it might be fairly assumed that if the solid were not volatile at the temperature to which it was exposed, it would be incapable of assuming the gaseous condition, and that it would therefore be abandoned by the solvent: hence, when the menstruum passed through the critical state, and became gaseous, the dissolved body would be precipitated in a solid form.

Such an assumption, however, was flatly contradicted by experiment. It was soon found that in many cases the solid

body was *not* deposited, but remained in a state of solution or diffusion in the gas. We are thus brought in contact with the unexpected phenomenon of a solid substance being *dissolved by a gas*, just as it might under ordinary circumstances be dissolved by a liquid.

Since water is the most generally useful solvent, it might be supposed that such experiments would be best made with aqueous solutions. Practically, however, the use of water is precluded, on account partly of its inconveniently high critical point, and partly of the fact that water at a high temperature and under great pressure is capable of exerting a corrosive action upon the glass tubes in which the experiments are undertaken. A more convenient solvent was found in alcohol, and many of the early experiments of Messrs. Hannay and Hogarth were made with a solution of iodide of potassium in this menstruum. A strong tube was about half filled with an alcoholic solution of potassic iodide; the extremity was sealed, the tube placed in an air-bath and heat applied. Having passed through the critical stage, the alcohol became gaseous; but the iodide, instead of being precipitated, remained in solution in this gas. Even when the temperature rose to 380° C., or about 150° above the critical point, the alcohol-gas still asserted its solvent power over the solid salt. Moreover, by an ingenious arrangement, it became possible to expose a fragment of the iodide to the action of the gas without allowing it ever to come in contact with the liquid; yet the solid slowly disappeared, and was at length completely dissolved by the invisible solvent. But on rapidly releasing the gaseous solution from the pressure to which it had been exposed, the iodide was precipitated, either as a cloud of delicate snow-like crystals, or as a crystalline film, like hoar-frost, on the inside of the glass tube. On again increasing the pressure, however, the crystals were re-dissolved, and once more disappeared.

Here then a new light broke in upon the phenomenon of solution. Hitherto it had been supposed that only liquids possessed solvent powers, but Messrs. Hannay and Hogarth have now shown that gases also are similarly endowed. In short, these researches fortify the conclusion which Dr. Andrews had previously reached, that there is perfect continuity between the liquid and the gaseous conditions.

If such extraordinary solutions can be effected, what more natural than to inquire whether *carbon* could be caused to dissolve in some appropriate solvent? Carbon is a remarkably obstinate body, resisting all ordinary menstrua, such as acids and alkalies, alcohol and ether. It is worth noting, however, that molten cast-iron can dissolve carbon; and that when the metal cools the carbon is partially separated in crystalline

scales, resembling graphite. Such scales are known to workmen under the curious name of *kish*.

Every schoolboy knows now-a-days that carbon occurs in nature crystallized as two entirely distinct minerals: in the one form it is known as *graphite, plumbago*, or *black lead;* in the other form as *diamond*. Metallurgists, as just stated, are familiar with the artificial production of graphite, and this body has also been produced by certain chemical reactions; but the artificial crystallization of carbon in the form of diamond has heretofore invariably baffled the chemist.

While the air of Glasgow was filled with the rumours of Mr. Mactear's experiments, it was natural to turn to Messrs. Hannay and Hogarth's researches, if haply their new method of gaseous solution might lead us to the desired end. They found that when a solid is freed from its gaseous solvent, it is invariably deposited in a crystalline condition. Now, if carbon could be thus dissolved, there was, of course, the bare possibility that it might be deposited in the crystalline form of diamond.

On applying himself to this inviting problem, Mr. Hannay was disappointed to find that all the forms of carbon with which he experimented, such as graphite, or charcoal, or lamp-black, obstinately refused to yield to any of the solvents which he brought to the attack. It was clear, therefore, that if the problem was to be solved at all it must be solved in an indirect manner, and Mr. Hannay's ingenuity was equal to the occasion.

Carbon is remarkable for the multitude of volatile compounds which it is capable of forming with hydrogen. Now Mr. Hannay found that when a gas containing carbon and hydrogen is subjected to heat under great pressure in the presence of certain metals, such as magnesium or sodium, the hydrocarbon is broken up, and its hydrogen combines with the metal, while its carbon is set free. In order to command the high temperature and the intense pressure necessary for this reaction, Mr. Hannay employs wrought-iron tubes, about $3\frac{1}{2}$ inches in thickness, and yet these are frequently torn open in the course of the experiments.

It appeared probable that the carbon set free in this decomposition might, at the moment of its formation, or when in the *nascent* condition, be dissolved by the gas, and then, on a reduction of pressure, be precipitated in a crystalline condition. Mr. Hannay has found that in order to obtain the carbon in the required crystalline state it is necessary that a stable compound containing nitrogen be present. When these conditions were fulfilled, the operator had the satisfaction of

finding that some of the carbon which was set free actually crystallized in the form of diamond!

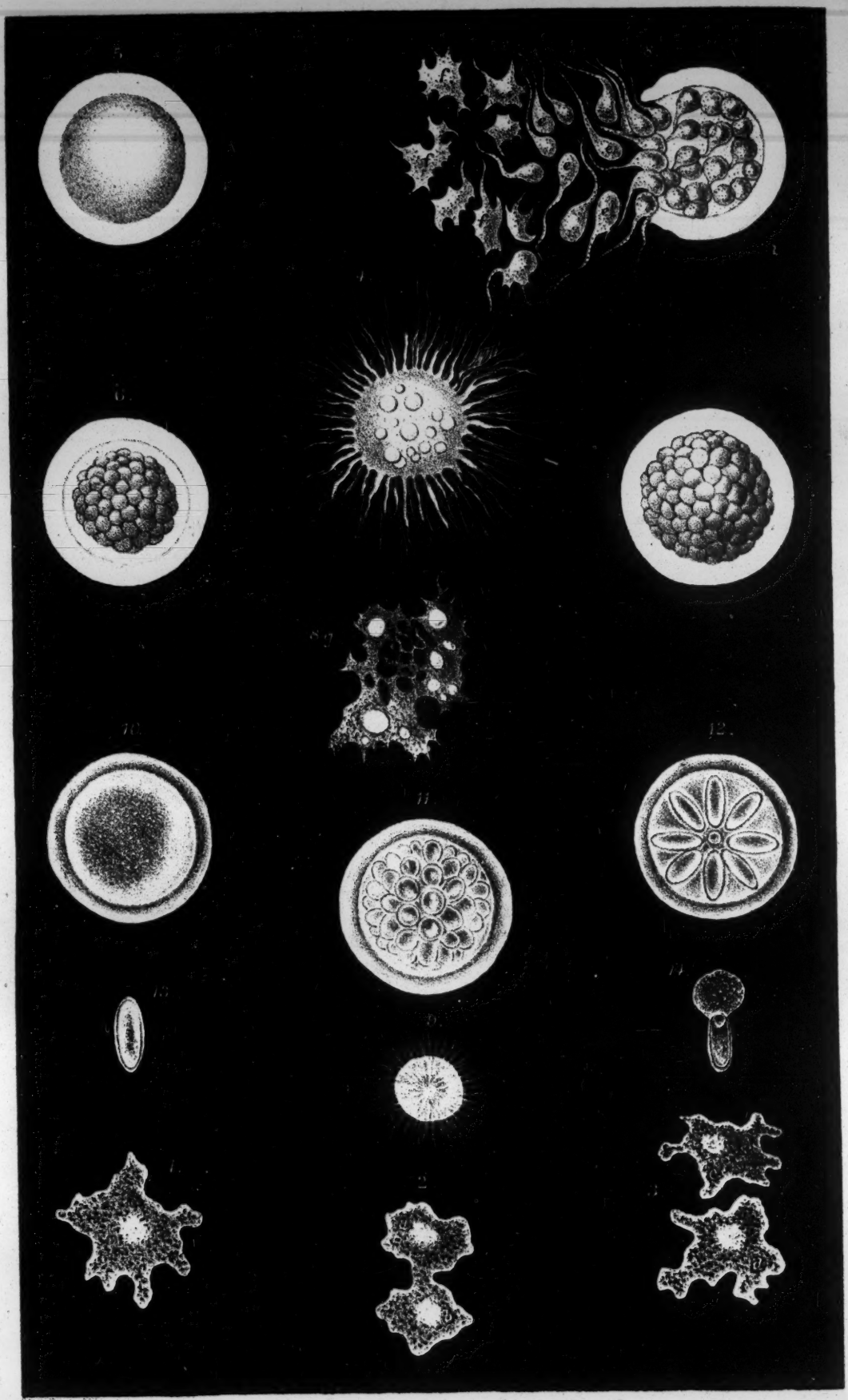
This adamantine carbon has been severely tested, not only by the discoverer himself, but also by so high a mineralogical authority as Professor Maskelyne. First, as to *hardness*, which of all characters is the most characteristic and the most valuable in diamonds: it is found that Mr. Hannay's crystals will easily scratch deep groves in a sapphire, and no substance save diamond possesses this strong abrading power. With regard to *crystalline form*, little can be said, but still that little is quite satisfactory. Perfect crystals have not, as yet, been obtained, and the fragments look like splinters of diamond rather than crystals. Still in one case Professor Maskelyne found traces of the distinctive octahedral cleavage, and Mr. Hannay has called attention to the curvature of some of the faces, so suggestive of diamond-crystals. *Optically* the crystalline fragments behave themselves just as diamonds might be expected to behave. Moreover, when placed in the scales they are not found wanting, for some of the artificial adamantoid carbon has as high a *specific gravity* as 3·5. Finally, the *chemical tests* leave nothing to be desired. Heated in the voltaic arc the carbon swells up and turns black, just as is the case with diamond; while if burnt in the usual way, in oxygen, it yields only carbonic acid; and though but a very small quantity was operated on, the result showed that the artificial crystalline body contained as much as 97·85 per cent of carbon. All the lines of evidence therefore converged to this point, that we are here dealing with a substance which is to all intents and purposes neither more nor less than *diamond*.

It thus appears that Mr. Hannay has mimicked Nature so successfully as to produce a body not distinguishable from the natural gem. In connexion with this interesting discovery, however, two questions naturally suggest themselves: first, has the artificial substance been produced in the same way as the native diamond? And, secondly, can the artificial product be made in such quantity and with such facility as to be profitably sent into the market?

The first question is by no means easily answered. Nature has such a wealth of resource at her command that in compassing a particular end she is by no means limited to a single method. Nothing is more likely than that the diamond has been formed in one way in this locality and in another way in that. In fact, the conditions of its occurrence are so dissimilar in different parts of the world as to make it highly probable that the diamonds of Brazil and the diamonds of South Africa have been brought forth by different processes. Mr. Hannay may or may not have hit upon an exact imitation of the natural

conditions of diamond-making, but even if he has gone to work after one of Nature's ways, it is far from necessary to assume that all diamonds have been fashioned in this particular manner. It was an old alchemical dogma that 'Vulcan is a second nature, imitating concisely what the first takes time and circuit to effect.' Obedient to this maxim, Mr. Hannay has pressed Vulcan into his service, but a good deal that we know about the natural diamond in certain localities tends to show that Vulcan has not always presided at its birth. Thus an eminent chemist recently said, 'We are entirely ignorant of the mode of its formation in Nature. The only thing which may be regarded as certain is that it has *not* been formed at a high temperature.'

After all, the genesis of the diamond is a subject of only scientific interest; the practical question for unscientific folk is whether Mr. Hannay can or cannot make his product in sufficient quantity to disturb the diamond market. Owners of gems, however, may be comforted by the assurance that, at present, the artificial specimens are small in size and costly to procure. When the chemist has completed his elaborate experiment, and opened the iron tube which has just been drawn from the furnace, he finds that his diamonds are not, like Sinbad's, 'of a surprising bigness.' They are, in fact, rather of a surprising smallness. But, were they no bigger than pins' heads, the experiment would still be a memorable scientific triumph. Practically, however, there is all the difference in the world between a laboratory experiment and a manufacturing industry. At the same time, it is of course possible that the recent Glasgow experiments may be merely the grains of mustard-seed which shall be eventually developed into a fruitful undertaking of commercial significance. Nothing, however, is more certain than that Mr. Hannay commenced his researches without the slightest regard to what Bacon calls 'the applying of knowledge to lucre.'



A. S. Foord, lith. Mintern Bros., imp.

THE THRESHOLD OF EVOLUTION.

By Surgeon-Major WALLICH, M.D.

[Plate IV.]

IN order to determine, with at least approximate certainty, the starting-point from which animal life took its earliest step in evolution, it is obviously essential that we should entertain accurate ideas as to the nature and characters of those humblest types of being that stand at the bottom of the series. Fifteen years ago it was the general belief amongst biologists that the Rhizopods, as presenting 'the distinctive attributes of animal life in their least specialized shape,' occupied the position. But, since then, scientific opinion has undergone an important, and, as I venture to think, prejudicial change, through the promulgation by Professor Hæckel of the hypothesis that there exists a third kingdom in creation, intermediate between the animal and vegetable kingdoms, the lowest group of which, *the Monera*, comprises 'not only the simplest, but the simplest conceivable organisms;' and hence, with a view 'to satisfy the requirements of the human understanding for causality,' we must accept as an 'unprovable,' but, nevertheless, absolute fact, that these organisms 'originated by spontaneous generation at the first beginning of life upon the earth.'

The chief aim of the following observations is to prove, on the basis of Hæckel's own descriptions and figures, that the structures in question have no *locus standi* as Monera; and, consequently, that we are as far off as ever from being able to say with confidence that they form the connecting link between the organic and inorganic world; or that they were 'the first molecules of matter that took upon themselves the responsibility of living.' *

* This expression was used satirically by Dr. Dawson, of Eozoon celebrity, in speaking of spontaneous generation.

Had Hæckel's statements concerning the Monera reached their climax when he made the extraordinary announcement that, in spite of each individual particle of which their bodies are composed having been proved, by the most refined chemical and optical tests, to be but an exact counterpart of every other particle entering into their composition—eight genera and sixteen species were readily distinguishable, a statement so tantamount to affirming that things which are equal to the same thing are NOT equal to each other, might safely have been left to find its own level. But when, in addition to this, an effort is made to upset every heretofore proposed classification of the Rhizopoda and Protozoa generally, on such untenable evidence as that upon which the existence of *Bathybius, Protamœba, Protomyxa, Protogenes,* and *Myxastrum*—all typical Monera be it observed—avowedly depends; and we are gravely asked to believe that '*the most remarkable of all Monera,* BATHYBIUS, probably even now always comes into existence by spontaneous generation,' in the depths of the ocean; it will, I think, be freely admitted that the matter demands a much more searching investigation than has hitherto been bestowed upon it.

I shall have occasion, hereafter, to allude to some of the minor evils resulting from this speculative style of teaching. But lest it be imagined that I am exaggerating the facts, I would invite attention to the two subjoined brief passages from the writings of Mr. St. George Mivart, which, though apparently written under a singularly erroneous view of the scope and limits of modern biological inquiry, unmistakably indicate that even amongst those who might reasonably be supposed to know better, 'the doctrines of evolution' and spontaneous generation are regarded as mere extensions of the same order of phenomena—relating, on the one hand, to organic life, and, on the other, to inorganic matter.

Thus, referring specially to Hæckel's observations upon the lowest forms of life, Mr. Mivart expatiates on the materialistic pantheism and the atheistic deductions from supposed facts which later investigations have proved to be fictions, '*e. g.*, the supposed organism *Bathybius Hæckelii*;' and declares that '*the doctrines of evolution logically culminate in three negations—namely, of God, of the soul, and of virtue.*'

Even Buchner, the earnest and undaunted advocate of freedom of thought and teaching, in his treatment of an essentially speculative department of knowledge, recognizes the necessity of rejecting mere hypothetical evidence in dealing with natural science. In his remarkable work, *Kraft und Stoff*,* whilst

* An excellent translation of Buchner's work, entitled *Force and Matter*, edited by Mr. T. F. Collingwood, F.G.S., has gone through two editions since 1864.

expressing the opinion that 'spontaneous generation undoubtedly played a more important part in the primeval period of the world's history than at present,' frankly allows, notwithstanding his unflinching faith in the potentialities of matter, that 'we possess no certainty nor well-founded data on the point, and are ready to confess our ignorance. But, though as regards organic creation much may be doubtful, we may positively assert that it may have, and has, *proceeded* without the interference of external forces.'

Now there is certainly nothing illogical in the opinion as thus stated, nor can it with fairness be looked upon as extreme even by the moderate Evolutionist. It is a simple avowal of the sufficiency of natural causes and conditions to account for the phenomena of life, *when once established*, and an outspoken warning that a rigid line of demarcation should always be drawn between that which admits of proof and that which at best can only be surmised.

Professor Virchow, another of Hæckel's countrymen and his quondam teacher, one of the most accomplished biological writers this century has produced, has yet more emphatically declared that where the object of a speaker or writer is to instruct, 'no hypotheses or assumptions, however dogmatically insisted on, can stand in the place of details, the results of observation, experiment, and thought.' It is, therefore, not surprising that Virchow should have vehemently attacked the Hæckelian system, or that two men with minds so antithetically constituted should thereupon have engaged in a controversy almost without parallel for the acerbity with which it has been conducted.

Let it not be imagined, however, that Hæckel makes any secret of his devotion to purely 'speculative science.' In his reply to Virchow, entitled *Freedom in Science and Teaching* (1879, p. 63), he says: 'In my opinion there is no boundary-line between the speculative departments of science and those that are actually conquered and firmly established; on the contrary, all human knowledge is subjective.' And in support of this proposition he then proceeds to show that even mathematics, which stands at the head of the exact sciences, is based entirely 'on those deepest and simplest fundamental axioms which are incapable of proof. It will be seen presently to what extravagant lengths 'freedom in teaching' may be carried where there is a preconceived hypothesis to support.

According to Hæckel, the Monera stand at the bottom of his list of *Protista*, which are *said* to constitute a very natural assemblage of organisms, presenting characters more or less common to animals and plants, but, in point of fact, comprising the *Foraminifera*, *Amœbæ*, and *Noctilucæ*, which are unquestionably animals; and the *Diatomaceæ*, '*Oscillarineæ*,' and certain

Fungi, which are as unquestionably plants. Whilst the *Gregarinæ* are made a sub-family of the *Amœbæ*, the *Amœbæ* are separated from the *Rhizopoda;* and the *Foraminifera*, *Heliozoa*, and already incongruous *Radiolaria*, are served up as an *olla podrida* under the name of the *Rhizopoda!*

But let us now direct our attention to the special group with which we are more immediately concerned, namely, the Monera.

With a marvellous eye to the value of precise dates, Hæckel tells us that the Monera originated in the beginning of the Laurentian period, 'by spontaneous generation as crystals from the matrix, out of simple inorganic combinations of carbon, oxygen, hydrogen, and nitrogen. If we tried to constitute, *a priori*, the simplest conceivable organisms, we should always be compelled to fall back upon such Monera. They are the simplest *permanent* Cytods. Their entire body consists of merely soft, structureless Plasson. However thoroughly we examine them with the help of the most delicate chemical reagents and the strongest optical instruments, we yet find *that all the parts are thoroughly homogeneous, each particle in the mass being a mere counterpart of every other particle composing it.* They are, therefore, in the strictest sense, organisms without organs, since they possess no organs, and are not composed of various particles. In a perfectly developed and freely mobile state, *they one and all present us with nothing but a simple lump of an albuminous combination of carbon. The individual genera and species differ only a little in the manner of propagation and development*, and in the way of taking nourishment. They can only be called organisms in so far as they are capable of exercising the phenomenon of organic life, of nutrition, reproduction, secretion, movement. Although in *all real Monera the body consists merely of such a living piece of Plasson, yet amongst the Monera which have been observed in the sea and fresh water, we have been able to distinguish eight different genera and sixteen species, varying in the mode in which their tiny bodies move and reproduce.* The extant Monera afford us organless organisms, such as must have originated by spontaneous generation at the first beginning of life upon the earth. *Even amongst the Monera at present known there is a species which probably even now always comes into existence by spontaneous generation*, namely, the wonderful *Bathybius Hæckelii. This has been proved to be* a necessary hypothesis, and is demanded by the requirements of the human understanding for causality. . . . The Monera may be fed with carmine or indigo powder if scattered in the drop of water under the microscope in which they are contained. The grains of colouring matter gradually penetrate the slimy body, and are then driven about in irregular directions. *The smallest separate particles or molecules of the Moneron-*

body, called Plastidules, displace each other, change their relative positions, and thus effect a change in the position of the colouring particles. This *change of position proves positively that a hidden structure does not exist.*' This, Hæckel affirms, 'is *a crushing answer* to the assertion that their organization is so minute that in consequence of the inadequate magnifying power of our glasses it is invisible.'*

It will be shown hereafter that the 'crushing answer' referred to by Hæckel is, in reality, no answer at all, inasmuch as a careful comparison of his descriptions and of the evidence furnished by his figures of the most typical Monera, suffice to prove that their body-substance is not homogeneous, even when seen under a very low magnifying power; but, on the contrary, consists of the usual pure basal protoplasm, thickly studded with minute granular particles, which are as integral a part of its composition, as they are of the protoplasm of every true mature Rhizopod without exception. But even were the alleged apparent homogeneousness and total absence of anything approaching to structure or texture in the protoplasm of the Monera a fact, we should be wrong in assuming the non-existence of all organization, inasmuch as it has been proved beyond all question by those indefatigable microscopists, Messrs. Dallinger and Drysdale, that, in a protoplasmic mass given off from the body of certain Monads, in which not a trace of structure or granularity was at first discoverable under the enormous magnifying power of a $\frac{1}{50}$th objective, after the lapse of several hours during which the object had not been lost sight of for a single moment, numberless extremely minute molecules made their appearance, and these proved to be fertilized germs, inasmuch as they continued to increase in size, and after a further lapse of time became developed into the mature parent form from which they had originated; the life-cycle of the species having been once more reproduced from the so-obtained mature forms. Here, then, we have incontestable evidence that germinal particles—*i.e.*, particles of matter—quite invisible in the earliest stage of existence, *must*, nevertheless, have been present in the protoplasm; and consequently that they must further be regarded as integral and essential factors in the life-cycle of the organisms which gave them birth.

Now, any person examining the figures of *Protamœba* in the accompanying plate (Pl. iv. figs. 1, 2, and 3) will at a glance perceive that they do not convey the impression of a perfectly

* For the full text of the above epitome I must refer the reader to the three latest works in which the questions now referred to are discussed, viz. Hæckel's *History of Creation*, published in 1876, *Evolution of Man*, 1879, and *Freedom of Science and Teaching*, also in the latter year.

homogeneous and structureless substance—such a substance, in short, as the pure albumen of an egg. On the contrary, the figures furnish the most striking evidence that the body substance contains distributed within it *except at the central part*, a multitude of minute but well-defined granular particles, of so nearly uniform size as to warrant the inference that they could not have been introduced accidentally into the bodies of the Monera, but form component portions of their structure. Concerning these particles I shall have something more to say presently. But meanwhile I must invite attention to another equally important proof of organization furnished by these figures.

At the central portion of Fig. 1, Plate iv., a circular space may be noticed, presenting fewer granular particles than the surrounding substance. The boundary of this space, though faint, suffices to prove that, like the granular particles, it is not an accidental portion of the structure. This alone is significant enough, but not so significant as the fact observable in Figs. 2 and 3. Fig. 2, as stated in the description, represents a *Protamœba* 'beginning to divide into two halves.' (It will have been noticed at p. 146, *ante*, that Hæckel speaks of this division as a process of reproduction). The central, comparatively clear space, shown in Fig. 1, has here already divided, one half being retained by and constituting the central space of the upper dividing half of the organism as shown at *a*; the other being retained by and constituting the central space of the lower dividing half as shown at *b*; the central spaces remaining clearly visible in Figs. 3, *c* and *d*, in which division is represented as having been completed.

But, according to Hæckel, 'originally, every organic cell is only a simple globule of mucus, *like a Moneron*, but differing from it in the fact that the homogeneous albuminous substance has separated itself *into two different parts*, a firmer albuminous body, the cell-kernel or nucleus; and an external softer albuminous body, the cell-substance or protoplasm. Besides this, many cells, later on, form a third, frequently absent, distinct part, inasmuch as they cover themselves with a capsule by exuding an outer pellicle, or cell-membrame.' (*History of Creation*, Vol. i. pp. 177–8). Here, then, we have Hæckel himself supplying the requisite data for a correct interpretation of the appearances presented by his *Protamœba*, and, as will presently be seen, his *Protomyxa* and *Myxastrum* likewise. The granules and the central clear space unmistakably indicate an already effected separation of the constituents of the protoplasm into two, if not three, parts, to what degree differing from each other, we have at present no means of accurately ascertaining, except from their external characters. But for our immediate purpose, it is quite sufficient to know that, since

a separation of the constituent atoms of any perfectly homogeneous substance into two or more parts, cannot possibly take place otherwise than through some molecular or chemical change in their original constitution, such substance can no longer be said to be so homogeneous that every individual particle is an exact counterpart of every other particle of the mass. The entire definition and character based upon it is therefore demolished, and with it the foundations upon which the Hæckelian hypothesis of the Monera has been made to rest.

Now in the early stage of every true *Amœba* without exception, granular particles, which undoubtedly constitute an integral portion of the organism even at this period, may be discovered scattered through its substance and taking part in the movements of the protoplasm, arising not from any circulatory faculty resident within it, but from the changes of form undergone by the animal in throwing out pseudopodia for the purpose of creeping or rolling itself along the surface upon which it happens, for the time being, to be moving. This is rendered perfectly certain by the circumstance that when the body ceases to move, the *quasi*-circulation ceases also. In the free-floating Rhizopods, as, for example, the *Foraminifera*, *Polycystina*, *Acanthometræ*, and *Dictyochidæ*, the same thing may be observed, though much less frequently, because in order to bring every portion of the body-substance in turn under the influence of the medium in which the organism lives, a very minute portion being exposed at a time beyond the shell or membranous covering, the vital contractility of the protoplasm is almost continually forcing some of the body-substance out of the foramina, and retracting within the shell a corresponding quantity. But where a separation of the protoplasm has taken place into a clear portion nearly devoid of granules, or, as sometimes is the case, more highly charged with them, it is always possible by dint of a little trouble to perceive that the central mass retains its character unchanged, there being, apparently, no longer a continuous interchange of protoplasmic substance between the central or nuclear portion and that which surrounds it. In the whole of the Rhizopoda the minute granules undoubtedly go to form the true nucleus when this organ is fully developed, and in the lowest order, in which no definite *single* nucleus is present, to form the smaller reproductive organs, to which I gave the name of Sarcoblasts, as being the bodies from which the young of the species are developed, both Nuclei and Sarcoblasts are almost entirely made up of the granules, which are, however, too minute to admit of their structure being resolved. But if the still more minute granules seen by Messrs. Dallinger and Drysdale proved to be the germs

of the parent Monad into which they eventually developed, it is perfectly legitimate to assume, as I did in the case of the Amœban Rhizopods, that the granules are the true reproductive elements of these organisms.

As regards the central clear space in Hæckel's figures of *Protamœba*, it is not requisite to offer many further observations. It is a well-known fact, and one repeatedly alluded to by Hæckel, that in all the more highly organized Protozoa the nuclear body is the first portion of the structure to undergo subdivision. But even allowing for argument's sake that the central space indicated in each figure of *Protamœba* was not intended to represent a nucleus, but some accidental displacement, caused by pressure or otherwise, of the granules pervading the substance of the body, the chances are enormously against such a displacement having taken place in each of the specimens figured. And it is equally improbable that in the representation of the *Protamœba* 'beginning to divide' into two halves, and of the two halves in which division had been completed, any such accidental cause should have led to a central clear space being present in *both* the *new* individuals, precisely as we see in the case of a true nucleus. Such an explanation is, therefore, inadmissible. Hence, as in the case of the manifestly 'differentiated' granular particles, we must regard the appearances as putting beyond all reasonable doubt the fact that the protoplasm of *Protamœba* is not what Hæckel assumes it to be, namely, a substance so homogeneous and structureless that every individual particle of it is the exact counterpart of every other particle composing it, there being no two different portions in the organism. Now the differentiation of living body-substance into two portions is a character on which Hæckel himself very justly lays the greatest emphasis, since it undoubtedly furnishes the most important proof available of advance from a lower to a higher degree of organization. Referring to this, he says:—'We must assume *two very different stages of Evolution in those elementary organisms which, as formative particles or plastids, represent organic individuality of the highest order.* The older and lower stage being that of Cytods, *in which the whole body consists of but one kind of albuminous substance of the simplest plasson or formative material;* the more recent or higher stage being that of cells in which a separation or differentiation of the original *plasma* into two different kinds of albuminous substances, into the inner cell-kernel or *nucleus*, and outer cell-substance or *protoplasm*, has already taken place.'—*Evolution of Man*, vol. ii. p. 45.

But the complicated web of contradictory statements respecting the Monera is not yet exhausted. In his *Evolution of Man*, Hæckel makes the definite assertion that 'the *Monera*, in

their HIGHEST stage of development, consist merely of small pieces of structureless *plasson* or slime;' and that 'the whole body consists merely of *plasson*.' (*Op. cit.* vol. ii. p. 43.)

Let us see how these remarkable statements tally with the history so circumstantially recorded by Hæckel of two of his typical Monera, *Protomyxa aurantiaca* and *Myxastrum radiale*.* It is almost superfluous for me to mention that in the earliest period of independent existence of nearly all the Protozoa holding higher rank than the Rhizopoda, there occurs an Amœboid stage, which is transitory and followed by the other developmental phases constituting the life-cycle of these organisms. Hæckel, on the other hand, draws the primary characters of his Monera—those characters upon which he bases the assertion that they are the simplest conceivable organisms —on the appearances presented by only one out of several phases through which they pass.

After premising that he is about to restrict himself entirely to 'true Monera, *i.e.* naked protoplasmic bodies without nuclei,' and means to pay no attention to the *Protoplasta* distinguished by the possession of one or more nuclei (as *Amœba* and *Arcella*), '*or* to the *Rhizopoda, distinguished by the possession of a distinct shell or membrane,*' we have presented to us a figure of the *Amœboid 'full-grown fasting'* stage of *Protomyxa aurantiaca* (See Plate iv. fig. 4), an organism detected by Hæckel upon a *Spirula* shell in 1866 at Lanzarote, one of the Canary Islands. Its red colour he regards as distinguishing it from other Monera. He traced it through the following distinct successive stages. The first, that of 'a minute tolerably opaque red ball, covered with *a thick, structureless membrane,*' nevertheless, subsequently found to consist of 'several concentric layers.' The orange-red 'contents of the balls appeared *as a thoroughly* HOMOGENEOUS *obscurely granulated mass, in which might be observed very numerous exceedingly fine particles and a small quantity of strongly-refracting red grains.*' (Plate iv. fig. 5.) Certain indentations on the surface of some of the balls proved to be the external sign of the breaking up of the whole cell-contents into a great number of smaller balls. In some of the balls which were kept under observations in watch-glasses filled with sea-water, the orange-red plasma divided as before into a number of smaller red balls. (Figs. 6 and 7.) But this division did not stop with a repeated bipartite division within the larger cell

* I purposely leave out of consideration Hæckel's Moneron *Protogenes primordialis,* as he admits it to be very closely related to *Amœba porrecta* of Schultze; adding that as the history of its mode of development and reproduction *was unknown to him,* he could not describe its affinities. There can hardly be any doubt as to its being Schultze's *A. porrecta.* Of *Bathybius* as a Moneron nothing need be said.

membrane, but went on, and this, Hæckel observes, '*would be better conceived as a* GERM-*formation than as a process of division or gemmation.*' Still later, some of the smaller or contained balls became pear-shaped and moved actively about when the cell-membrane burst, their movements being accelerated as soon as they were free. (Fig. 8, *e.*) Ultimately, 'the pear-shaped *tail-bearing* GERMS, *or rather germ cytods*' (whatever that may mean), assumed an *Amœboid* condition (Fig. 8, *f*), which Hæckel again declares was structureless throughout, and '*thereby the morphological status of* THE SIMPLEST CONCEIVABLE organic individual was that of a naked cytode or gymnocytode.'

'*Myxastrum radians*' (Plate iv. fig. 9), at first reminded Hæckel of '*Actinosphærium Eichhornii,*' inasmuch as it consisted of a small globular mass of *structureless, homogeneous* jelly, covered with the radiating pseudopodia of *Actinophrys.* The central or inner mass consisted of a sarcode body which contained very numerous interspersed, bright, shining particles, and a small number of larger strongly-refracting granules. *Myxastrum* is distinguished from *Actinosphærium* by the absence of vacuoles, nucleus-holding cells, and any difference between its outer and inner portions, the whole mass being homogeneous. On this ground it might rather be associated with *Actinophrys Sol.* But, again, it does not possess the contractile vesicle of the latter, '*and is especially distinguished by its peculiar reproduction.*' Hæckel attempts to account for the granules by regarding them as '*the products of a change of substance,* it being *extremely probable* that they are assimilated substances produced by the chemical action of the digestive sarcode upon the food taken, and are afterwards changed again into sarcode.' This is supposed to be proved by the increase or diminution in the number of the granules which were observable, as the creatures, whilst under observation, were either fed upon generous diet, or starved. After a time the small globular mass of jelly constituting the body of *Myxastrum* became encysted like *Protomyxa* (Fig. 10), the cyst-membrane becoming thicker and thicker by the addition of fresh concentric layers, and finally reached a thickness amounting to an eighth of the diameter of the entire mass. After the lapse of a fortnight the contents were developed into a number of spindle-shaped bodies (Figs. 11 and 12), which subsequently became oblong and covered by a thin wall having a well-defined double outline, which was *proved to consist of silex* (Fig. 13). Ultimately, the siliceous wall of each of these bodies burst at one extremity, and gave egress to minute spherules—'sporangia' (Fig. 14)—which finally took the parent form of *Myxastrum.*

Here, then, we have presented to us the clearest proof that

typical *Monera*, instead of being 'through life' nothing more than little lumps of the structureless, &c., protoplasm, and exhibiting 'no higher degree of development than that observable in pure protoplasm, undoubtedly pass through several stages in their life-cycle, and that in two at least of these stages, their organization is obviously of a complex kind, even when scrutinized in the most cursory manner. Thus we find they pass through an *Amœboid*, or *Actinophryan*-like stage, a stage of encystment, in which the contents of the cyst—itself a highly differentiated portion of the structure—undergo a process akin to segmentation; each segment, in the case of Protomyxa, emerging from the burst cyst in the shape of a pyriform, caudate, zoospore, which moves about actively, but ultimately takes on the Amœboid form; and in the case of Myxastrum, emerging in the shape of oblong *siliceous* cysts, which on bursting give egress to a little mass of protoplasm, which, as in *Protomyxa*, takes on the Amœboid (Actinophryan) character—a series of facts so significant, that I venture to say they prove incontestably that whatever may be the precise position in nature of the organisms described, they are not inferior in complexity of structure to the Rhizopods.

In short, the error committed throughout Hæckel's observations consists in the determination to regard the protoplasm of the organisms in question as perfectly homogeneous, because he considers this character essential to his hypothesis of their having been formed by spontaneous generation. The researches of Messrs. Dallinger and Drysdale, already referred to, clearly demonstrate this error. In the case of the Monera, which are giants in comparison with Messrs. Dallinger and Drysdale's Monads, the presence of granular particles suffices to upset the hypothesis based upon the absolute homogeneity of their protoplasm, in spite of Hæckel's effort to explain away their undeniable presence on the ground that they are 'the products of a change of substance—assimilated matter, to be eventually changed into sarcode.' For, what are blood-corpuscules, spermatozoa, and every tissue in an animal's body, but products of a change of substance?

Did space permit, it would be easy to show that in the *Amœba*, when immature, the nuclear body not unfrequently escapes observation, owing either to its transparency or to the presence of the granular particles, as well as food-particles, which make their appearance in the sarcode at a very early stage in the life-history of these organisms. The nucleus is, however, always present when *reproductive division* (a term I now use merely to distinguish it from simple repetitive division) has taken place; inasmuch as the former process cannot occur unless a nucleus is present; whereas simple division into

two halves may be repeated almost indefinitely in the absence of a nucleus.

Were proof needed of the difficulty sometimes experienced in detecting the nuclear body, it is to be found in the fact that *Gromia*, one of the most highly developed Rhizopoda, was classed not only *with* the Foraminifera in the lowest order of these organisms until the year 1864, when they were shown by me to be furnished with both a definite, encapsuled nucleus, and a contractile vesicle, but was retained as the *type* of Foraminiferal structure in almost every text-book on the Protozoa.

I may be allowed to say, in conclusion, that I do not pretend to be able to determine the exact position and relations of the structures described as Monera. In order to do this it would be requisite to examine living and authenticated specimens. But I am much mistaken if any such determination is needed to demonstrate that the Monera do not exist in nature as a group of organisms holding an intermediate position between true animals and true plants, as laid down by their describer. They must, accordingly, be henceforth relegated to that limbo of departed spirits to which their illustrious congener, *Bathybius*, has already preceded them—unless, indeed, we are to accept another of the extraordinary conclusions arrived at by Professor Hæckel in relation to the Monera, viz., that 'the *conception of an independent soul-life to every individual organic cell is a validly proved conception, by the study of Infusoria, Amœbæ, and other one-celled organisms; for in these individual isolated living cells, we find the same manifestations of soul, life, feelings, and ideas (mental images), will, and motion, as in the higher animals compounded of many cells.*' (*Freedom of Science and Teaching*, p. 47.)

EXPLANATION OF PLATE IV.

The whole of these figures are, as nearly as possible, facsimiles of Hæckel's originals.

FIG. 1. *Protamœba primitiva*, a typical freshwater Moneron, in the free Amœboid condition.

FIG. 2. The same beginning to divide. It will be seen that the central clear space visible in fig. 1, has already divided, and is visible both at *a* and *b*.

FIG. 3. The same, division being complete; and the two separate individuals each presenting a central clear space.

FIG. 4. *Protomyxa aurantiaca*, another typical Moneron. 'A full-grown specimen' said to be 'fasting.'

FIG. 5. The same 'encysted.'

FIG. 6. The same, more developed. 'The homogeneous protoplasm has

retracted itself from the inside of the wall of the cyst, which has become thickened, and has begun to divide into numerous small globular bodies. Between the plasma-ball and the outer covering a little clear fluid has collected.'

FIG. 7. The same further developed.

FIG. 8. 'The small globular bodies have now drawn themselves out into a long tail, and issue from the cyst as *swarm-spores*, with a lively motion.' At *e*, 'the pear-shaped spores, having ruptured the cyst, move about freely with the aid of their tails.' At *f*, some 'spores becoming stationary retract their tails and protrude pseudopodia,' by means of which they creep about after the manner of *Amœbæ*. Fig. 8, *g*, shows 'three of these Amœboid germs united into a single plasmodium;' vacuoli being now for the first time observable.

FIG. 9. *Myxastrum radians*, another typical Moneron in the Amœboid or Actinophryan stage: not quite full-grown.

FIG. 10. The same after arriving at full growth, encysted, 'showing the homogeneous colourless ball of protoplasm, surrounded by a *tough, structureless, gelatinous*, covering.'

FIG. 11. The same, 'at commencement of development, dividing by *radial cleavage* into numerous conical portions.'

FIG. 12. The same, still further developed. 'The cone-shaped masses have now assumed a spindle-shape, and each separate one has developed a *siliceous* covering.'

FIG. 13. The empty siliceous shell of one of these '*spores*.'

FIG. 14. 'A spore, showing the protoplasmic contents escaping from the siliceous shell.'

REVIEWS.

BRITISH MARINE POLYZOA.*

EVERY naturalist knows the magnificent series of works on British Zoology for the publication of which we are indebted to the liberality and enterprise of Mr. Van Voorst. The Quadrupeds, Reptiles, and Stalk-eyed Crustacea of the late Professor Bell, whose recent death at a ripe old age is deplored by all who knew him; Yarrell's Birds and Fishes; the History of British Echinodermata, by Professor Edward Forbes, and the splendid work on the Mollusca, by the last-named writer and Mr. Sylvanus Hanley; and MM. Spence Bate and Westwood's history of the Sessile-eyed Crustacea, are all standard works, covering a great part of the domain of British Zoology, and reflecting the highest credit both upon their authors and upon the publisher who has brought them out so handsomely printed and so admirably and lavishly illustrated. Besides the above-mentioned works, Dr. George Johnston, of Berwick, one of the best naturalists of his day, contributed to the series in 1849 a second edition of his History of British Zoophytes (originally published in Edinburgh in 1838), in which he included the whole of the animals to which the term Zoophyte was then commonly applied, namely, the Sea-anemones and Corals, and the Alcyonarian and Hydroid Polypes, belonging to the division Cœlenterata of more recent zoologists, and the Polyzoa, or so-called Ascidian polypes of some authors of that day, which had been regarded as for the most part allied to the hydroid Sertularians by the older zoologists, until the distinction between the two forms was pointed out by Grant and Milne-Edwards, followed about 1830 by Vaughan Thompson and Ehrenberg, and a little later by Dr. Arthur Farre.

As a matter of course, in the thirty years which have elapsed since the publication of the last edition of Dr. Johnston's work, much has been done in the study of a group of organisms so interesting to zoologists as these lowly creatures. Many new forms have been discovered, many points in their history have been cleared up, and new, unexpected, and most remarkable phenomena have been observed by the unwearied investigators of the last quarter of a century. Mr. Gosse's useful and beautifully-illustrated History of British Sea-anemones and Corals was the outcome of the general attention paid to these flowers of the sea in consequence of the setting in of the fashion

* *A History of the British Marine Polyzoa.* By Thomas Hincks, B.A., F.R.S. 2 vols. 8vo. London, Van Voorst, 1880.

of keeping aquaria; the Alcyonarian zoophytes are still waiting for their historian, but the remainder of the forms properly included under the term Zoophytes by Johnston and the older naturalists, have now met with worthy treatment at the hands of the Rev. Thomas Hincks, whose valuable treatise on the British Hydroid Zoophytes appeared in 1868, and is now followed by a splendid work on the Marine Polyzoa. The few fresh-water species are omitted, as having been made the subject of a special treatise by Dr. Allman, published by the Ray Society.

The Polyzoa, which include among others the well-known *Flustræ*, or sea-mats, and many of the slender, branching, horny zoophytes which enter into the composition of the so-called sea-weed baskets and pictures familiar to visitors to our watering-places, were regarded by Linnæus and his successors, down to the time of Lamarck and Cuvier, as very nearly related to the *Sertulariæ* and other Hydroid polypes, the last-named author, in the second edition of his *Règne Animal* (1829), only separating them from the latter (his *Polypes vaginiformes*) as a distinct family under the name of 'Polypes à cellules,' the distinction being founded on differences of the polypidom, and the animals in both cases said to resemble *Hydræ*, although the greater simplicity of the Sertularian polype seems already to have struck Cuvier. But at an earlier period, it had been noticed that the differences between the animals of different species of so-called *Sertulariæ* were very considerable. Thompson's observations were not published until 1830, but he states that they were made soon after 1820. He called the newly-recognized form of polype a 'Polyzoa,' in opposition to the term 'Hydra' applied to the individual zooids of the true Sertularians and their allies. In the meantime, however, the distinction had been noticed by Grant and Milne Edwards; the fact that the new type of structure could be assimilated to that of the Ascidians was recognized, and shortly afterwards the group was removed from the division of Zoophytes to that of the Mollusca, and placed in the immediate vicinity of the Ascidians. Of late years there has been considerable discussion as to the propriety of the position thus assigned to the Polyzoa, several eminent authorities being inclined to remove them to the Vermes, under which rather miscellaneous head it seems difficult to find their allies; whilst others still maintain their molluscan affinities. To the latter opinion Mr. Hincks adheres, and he strengthens his point here by adducing what appears to be a strict homology between the Polyzoon and the embryo of the bivalve Mollusca.

Another matter about which there has been a considerable amount of dispute is the name which ought to be given to this class of animals. Thompson's memoir was published in December, 1830, and in June, 1831, appeared the first part of Ehrenberg's *Symbolæ Physicæ*, in which the Prussian naturalist established a class, 'Bryozoa,' for the reception of the Polyzoa and of some other organisms, which he associated with them under a mistaken notion of their affinities. The question that has arisen is as to which of these names should be adopted. On the mere question of priority there need be no doubt, always assuming that Thompson proposed the formation of a new group of zoophytes under the name of Polyzoa; otherwise 'Bryozoa,' if admissible on other grounds, will be the earliest name applied to the class. The objection raised to 'Polyzoa' is that it was used

by Thompson in the singular number, and he speaks of it as 'a new animal discovered as an inhabitant of certain zoophytes;' and he afterwards says of the *Flustræ* that in many of them he had 'clearly ascertained the animals to be *Polyzoœ*.' Mr. Hincks maintains what is perfectly clear, that Thompson 'used the term *Polyzoa* (in opposition to *Hydra*) to denote *a distinct type of structure* which he had demonstrated;' but we cannot follow him in holding that it 'was not as the mere name of the single zooid,' or that '*Polyzoa*, as he (Thompson) uses it, is essentially a class-designation, and not the name of a mere structural element.' We must place ourselves in Thompson's position to understand precisely what he meant, and it seems to us that in the use of the term *Polyzoa*, in opposition to *Hydra*, he was merely following out the practice of some of the older systematists whose works he had studied, such as Linnæus, who uses the term *Hydra* to designate what he calls the 'flores' of these very zoophytes, just as he used the terms *Limax*, *Nereis*, *Tethys*, &c., to indicate the general structure of the animals of his Vermes Testacea. In this sense and no other can we understand the peculiar mode in which Thompson speaks of his 'Polyzoa.' The term is used by him to express a 'structural element' of the composite organism, and not as a class designation. In fact, after reading Thompson's memoir, it seems quite clear that he had not the slightest idea of founding a new and distinct class, or even order, of animals, as indeed may be seen from one of the passages quoted by Mr. Hincks in support of his own opinion, in which Thompson says, 'The *Polyzoa* will probably be found in many dissimilar genera of the zoophytes, and even mixed up with the *Hydra* in some, ... and hence this discovery must be the cause of extensive alterations and dismemberments in the class with which they have hitherto been associated.'

Professor Rupert Jones puts the matter in another light, and holds that the class Bryozoa of Ehrenberg contains such incongruous elements (the Foraminifera forming one great division of it) that the adoption of this name is inadmissible, and that we must therefore discard it altogether and accept the name of Polyzoa for the class on the authority of Mr. Busk, who first made use of it as a definite class-designation. But it is to be observed, on the other hand, that the term Bryozoa was in use as the name of a class restricted nearly as at present many years before the adoption of the term Polyzoa by Mr. Busk, and this would again cause the scale to descend in favour of the former name. Moreover, Ehrenberg himself did not at first include the Foraminifera under Bryozoa.

We have said so much about the Polyzoa and their name, that our reference to the book which has served as the text of this sermon can only be a brief one. This, however, is of the less consequence, as the only terms in which it can be spoken of are those of unmixed praise, and such laudations may be given in comparatively few words. The great body of Mr. Hincks' work consists of the systematic description of the species of British Marine Polyzoa, of which he recognizes 235, many of them common on different parts of our coasts, others only obtained in certain places, or by dredging at considerable depths. The characters of these species, and of the genera, families, and higher groups to which they are referred, are most carefully described and discussed. Mr. Hincks has devoted particular attention to the varieties displayed by many species, which, as he justly indicates, have

acquired a special interest in consequence of the direction taken of late years by biological research. Under each species, besides the descriptions and remarks on the characters, we find a full synonymy, a statement of the mode of occurrence of the species, a list of the British localities where it occurs, a full indication of its geographical distribution, and in the case of those species which occur fossil, of its range in geological time. In fact, Mr. Hincks seems to have aimed successfully at making the systematic portion of his work as complete as possible. The student who makes use of this book for the systematic investigation of our Marine Polyzoa will doubtless find his work comparatively easy, but the subject in itself is by no means an easy one; the researches necessary for the production of the present volume must have been long-continued and environed with many special difficulties, and we may heartily congratulate Mr. Hincks on the results of his long and arduous labours.

In his Introduction, which extends to over 140 pages, and deals with the generalities of the class, its structural characters in general, the production of special parts, such as the curious avicularia, or 'bird's heads,' and the vibracula, and the physiology of the animals, including their reproduction and embryology, we find evidences of the same conscientious care on the part of the author, who cites and discusses the statements contained in all the most recent publications on these points, selecting and supplementing them by the light of his own researches. This portion of the work is as admirably done in its way as the systematic part, and will serve, not only as a necessary guide to the proper understanding of the latter, but also as a good summary of the anatomy and physiology of the class.

The illustrations consist of 83 plates, forming the second volume of the book, and of numerous woodcuts scattered through the text of the first volume. The Introduction is almost entirely illustrated with wood-engravings, although here and there a plate is referred to, but for the elucidation of the development of the Bryozoa the author has given a plate of beautiful figures, copied from the magnificent work of Dr. J. Barrois, of Lille, on the embryology of these animals. The rest of the plates, illustrative of the systematic section of the book, have been beautifully executed in lithography by Mr. Hollick, and with the aid of these and the author's clear and precise descriptions, the determination of the species of British Marine Polyzoa ought certainly henceforward to present little difficulty. All the woodcuts, which are distributed more or less abundantly throughout the volume of text, are most delicate and beautiful specimens of the art.

THE CRAYFISH.*

THE Crayfish is one of the type animals described by Professor Huxley and Mr. Martin in their *Elementary Biology*, and it is also, we believe, one of those the structure of which is always investigated by the former

* *The Crayfish, an Introduction to the Study of Zoology.* By T. H. Huxley, F.R.S. Small 8vo. C. Kegan, Paul & Co. (International Scientific Series), London, 1880.

gentleman's students during their course of instruction. Hence we may take it for granted that Prof. Huxley is pretty well up in everything relating to Crayfishes; and when we consider the position of that type in the series of animals, it is easy to understand why our author regards it as specially adapted to furnish the text for an 'introduction to the Study of Zoology.'

In his little volume on the Crayfish just published as one of the 'International Scientific Series,' Prof. Huxley describes in the first place the natural history, structure, and physiology of that interesting little Crustacean, and then proceeds to show what lessons in general zoology may be learned by the intelligent application of the special knowledge thus acquired. His first chapter is devoted to the natural history of the animal, and includes neces arily a general account of its external structure. The second and third chapters deal with the physiology of the Crayfish, and include the description of its internal anatomy; the fourth treats of the morphology and development of the animal; and the fifth of its comparative morphology, its relationship to other living forms, and the general zoological teaching to be derived from the consideration of the results of the special study of its structure. The line of thought started in this last chapter is continued in the sixth and final one, in which the author discusses the distribution and ætiology of the group of Crayfishes, and indicates the bearing of investigations, such as those to which the previous chapters are devoted, upon those higher questions of biological philosophy, the origin of species, and the causes of the relationships existing between the members of groups of organisms, and between these groups themselves. The whole subject is developed in the most admirable manner, and we may fully grant the author's claim on behalf of his book that 'whoever will follow its pages, crayfish in hand, and will try to verify for himself the statements which it contains, will find himself brought face to face with all the great zoological questions which excite so lively an interest at the present day.'

We have yet a word to say about the illustrations, which are numerous, well adapted to their purpose, and exceedingly good; most of the figures of Crustacea, and especially those of the Crab and of the species of Lobster, are indeed very beautiful specimens of wood-engraving.

A BIOLOGICAL ATLAS.*

SIDE by side with Prof. Huxley's Monograph on the Crayfish, we may notice MM. M'Alpine's *Biological Atlas*, as this is to be regarded to a great extent as illustrative of another book of which the Professor is part author, namely, the *Elementary Biology* published by him and Mr. H. N. Martin in 1875. That excellent little work was in point of fact the outcome

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* *Biological Atlas, a Guide to the Practical Study of Plants and Animals, adapted to the requirements of the London University, Science and Art Department, and for use in Schools and Colleges, with Accompanying Text, containing arrangement and explanation, equivalent terms, glossary, and classification.* By D. M'Alpine and A. N. M'Alpine. 4to. W. & A. K. Johnston, Edinburgh and London. 1880.

of the course of biological instruction given by Professor Huxley to his students in his laboratory at South Kensington, and it described the characters and methods of investigation of a selection of plants and animals which might serve 'to exemplify the leading modifications of structure which are met with in the vegetable and animal worlds.' The organisms selected were the following:—The Yeast-plant, *Protococcus*, the Proteus-animalcule (in connexion with which the phenomena presented by colourless blood-corpuscles were noticed), Bacteria, Moulds, *Chara*, the Bracken Fern, the Bean-plant, Bell-animalcules, *Hydra*, the Freshwater Mussel, the Crayfish and Lobster, and the Frog. Of all these types the external structure and life-history were indicated, the anatomy and physiology described, and the mode of investigating their structure and functions noticed in detail, but the text was not accompanied by any illustrations. This want is to some extent now supplied by MM. M'Alpine's *Atlas*, which contains a series of coloured plates showing the characters and structure of the organisms above cited, and illustrating the results of practical work carried out in accordance with the directions given in the *Elementary Biology*. The authors of the *Atlas* have, however, added slightly to the compass of the descriptive work, by the introduction of the Horse-chestnut as a second flowering plant, and of the Earthworm and Snail as additions to the series of animals illustrated, and these latter augmentations of the original plan are certainly very judicious. We think that the further addition of an air-breathing Arthropod, especially of a representative of the Insecta, would have been advantageous.

The twenty-four plates composing this Atlas contain figures of the organisms above referred to and of their organization as displayed by the processes of dissection applicable to them. They are printed in colours, the colours being used for the purpose of distinguishing the various organs or systems of organs. Hence the figures are necessarily diagrammatic, but although rough, and not always quite so accurate as they ought to be, they may be of considerable service to biological students, especially to those who are attempting to gain a knowledge of the structure of organisms by the process of self-instruction. We notice one other defect, namely, that in many cases the scale of the enlarged figures is not indicated. This is a point of great importance for beginners.

Each plate is accompanied by a leaf of explanatory letter-press, which gives the necessary information as to the significance of the figures. At the beginning of the book there are also a few pages of general directions for practical work, indicating the instruments and reagents required for carrying out the course of study illustrated by the plates, and also the mode of procuring and examining the various organisms referred to.

BOTANY FOR CHILDREN.*

EVERY one knows how the late Professor Henslow, in civilizing what was said to be, when he took it, one of the roughest parishes in

* *Botany for Children.* An illustrated Elementary Text-book for Junior Classes and Young Children. By the Rev. George Henslow, M.A., F.L.S., &c. Sm. 8vo. London: E. Stanford, 1880.

England, found the practical instruction of the school-children in Botany exert a most ameliorative influence upon their minds. His son, the Rev. George Henslow, anxious to place in the hands of country clergymen and school-teachers the means of employing the same method of cultivation, has written a little book of practical instructions under the title of *Botany for Children*, which, he hopes, may serve to indicate to teachers the mode of leading their younger pupils to a comprehension of the simpler facts of Botany, and to guide children of larger growth in their first steps in the study of the science. For this purpose he has selected a certain number of common flowering field-plants, belonging to various natural orders, and described in the plainest possible terms the mode in which their structure should be investigated, indicating at the same time the way in which the facts thus ascertained are applied to the classification of plants, explaining the physiological functions of the various parts, and very briefly discussing the general principles of vegetable morphology. The choice of plants for investigation seems to have been very judiciously made, although it is, perhaps, to be regretted that a few more orders are not represented. We notice especially that no example is cited of so important a group as the Umbelliferæ, which surely ought to have received some notice, many of its members being such striking objects that the young botanist can hardly help wishing to know something about them, whilst their structure is remarkable and exceedingly interesting from a morphological point of view. The Urticaceæ, Papaveraceæ, Dipsaceæ, and some other groups, are in the same case. Nevertheless, the readers for whose advantage Mr. Henslow's little book has been prepared, have every reason to be grateful to him for the trouble he has taken to make the path before them smooth and easy for their first uncertain steps; and we may say with confidence, that any young botanist who carefully follows out in practice the course of study here laid down, will arrive at the close of his first summer with no inconsiderable amount of *precise* knowledge, which will render his further progress comparatively easy. We may add, that the little volume is illustrated with a series of very characteristic figures of the plants noticed, with details of their floral structure, nicely executed in lithography, by the hand, we presume, of the author himself.

INTELLECT IN ANIMALS.*

WITH the revival of the doctrine of the origin of species by a process of evolution, which was brought about by the publication of Mr. Darwin's celebrated work on the subject, the relation of man to other organisms at once became a question of great prominence. It was evident that, man being physically an indubitable mammal, he must, on this side of his nature at any rate, be regarded as subject to the same laws as the rest of the organic world, and the expressed or implied recognition of this fact certainly gave rise to much of the opposition with which the new

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* *Mind in the Lower Animals, in Health and Disease.* By W. Lauder Lindsay, M.D. 2 vols., 8vo. C. Kegan Paul & Co., London, 1879.

doctrine had to contend. People who were quite contented to speak of the apes as 'our cousins,' and to regard them as unquestionably the nearest relations of mankind, taking those words in a merely classificational sense, and who, on the other hand, would probably have had no objection to accept the theory of evolution if confined to plants and the lower animals, cried out loudly against the new view as soon as it appeared clearly that if they were true for one series of organisms they must apply equally to the whole. 'The dignity of man' was said to be gravely compromised by any such admission as this; and it will be remembered by many that this sentiment was the engine principally employed to bring the general public into a state of feeling antagonistic to any hypothesis of evolution.

Side by side with these considerations, founded on the so-called 'dignity of man,' and, indeed, to a considerable extent springing from them, or forming part of them, came certain religious or theological objections, which were not without considerable influence. Divines and religious writers had for many years held it as a fixed principle, not only that man was the only reasoning being, but that his rationality was due to the possession of a soul, an advantage said to be denied to all lower animals; and then, arguing somewhat in a circle, the exclusive possession of reason by man was taken as evidence of his having a soul. Hence it is easy to understand how the dispute as to the identity, or otherwise, of the mental phenomena of man and other animals, became really the turning-point in this discussion; for it is quite clear that, in case the identity was demonstrated, there would be no ground for denying souls to all animals (as indeed has been maintained by the Rev. J. G. Wood); or else the evidence hitherto relied on for demonstrating the existence of the soul in man would become nugatory, and such evidence would have to be sought somewhere else. With this, however, we have nothing to do; the only object of the preceding remarks being to indicate the principal reasons which have caused such attention to be given of late years to the comparison of the mental operations of man and animals.

In two of his works: *On the Descent of Man*, and *On the Expression of the Emotions in Man and Animals*, Mr. Darwin has himself treated more or less fully upon these matters; and the question will be found handled in greater or less detail in nearly all the numerous books in which the possible origin of man by evolution from some lower type of organism has come under consideration. The Rev. J. G. Wood has devoted a special work to the attempted demonstration of the precise agreement between the mental phenomena of man and animals (*Man and Beast*, 2 vols., 1874); but he tacks on to his argument in this direction a somewhat unfortunate corollary, namely, that this identity being proved, it follows that all animals, as well as man, possess immortal souls.

Upon the general question of the identity in kind of the mental operations of animals and men, it seems to us that no one possessing fair powers of observation and analysis who has ever made a friend of his dog can entertain the slightest doubt. It is not so much upon special instances of intelligence (of which we have had rather a surfeit in the columns of *Nature* during the last two years), but rather upon the general behaviour of the animal, his daily *Leben und Treiben*, that we would rely. In all such an animal's ordinary actions we have examples of reasoning, at least as

profound as the faculty manifested by young children and some of the lower races of men, whose most abstract reasoning always relates solely to their material wants.

In his book entitled *Mind in the Lower Animals*, the most elaborate work that has appeared upon the subject, Dr. Lauder Lindsay has followed out this line of research in a most complete fashion. Commencing with a general consideration of the principles of investigation of the subject of 'Comparative Psychology,' he points out the causes of erroneous notions upon this matter, and the method of study to be pursued, discusses the gradual evolution of mind in the ascending zoological scale, and certain other general matters, winding up with a very curious chapter on unsolved problems in the psychology of the lower animals. Proceeding then to the normal manifestations of mind in the lower animals, he indicates at starting that he must compare these not with the phenomena presented by adult cultivated people, but with the intellectual manifestations observed in 'early states or stages of human society, as illustrated by primitive or *savage man*,' and with 'the infant or *child* of civilized parents,' adding to these, however, idiots, insane people, criminals, with certain other conditions of men, cited for special purposes. It will be seen that Dr. Lindsay lays a very wide foundation for his arguments, but for practical purposes the first two categories, namely, the representatives of savage races, and the children of civilized peoples, are of the most importance, and indeed figure most prominently in all sections of the work. In these the author analyses and compares successively the mental phenomena of man and other animals under the heads of Morality and Religion, Education, Language, Adaptiveness, and Fallibility—the last named including the discussion of instances of errors committed by animals, deceptions practised and practical jokes performed by them; whilst under Adaptiveness we have notices of a peculiar series of phenomena, such as social organization, the existence of something like law and punishment, the use of instruments, clothing and shelter, the faculty of numeration and power of calculation.

It will be seen from this bare enumeration of subjects how thoroughly Dr. Lindsay has entered into the matter before him, and the multitude of citations which he makes throughout this portion of his work bears testimony to the conscientious labour which he has bestowed upon its preparation. If we were bound to find fault with it, we should say that it is too elaborate: the author seems to have aimed at saying everything that can be said on the subject, and in consequence dwells with what seems to be a rather unnecessary circumstantiality upon points which might perhaps have safely been indicated and then left to take care of themselves.

The outcome of the whole discussion is, we think, a complete demonstration of the absolute identity in kind of the normal mental manifestations of the lower animals with those presented by savage tribes and by the children of civilized peoples; but our author is not contented with this, and in a second section of his work goes on to show an almost equally close analogy between the abnormal mental phenomena in man and animals. There is much that is exceedingly curious and interesting in this part of his book, in the preparation of which the special attention paid by the author to insanity in the human subject has stood him in good stead; but the view maintained by him follows so naturally from the demonstration of the identity of the

phenomena under normal conditions, that we need not refer to it further here.

This portion of Dr. Lindsay's book is followed by an appendix containing a bibliography of the numerous works from which his facts have been derived, and elaborate tables of the genera and species of animals referred to in the text, the whole concluding with an elaborate analytical index, a most important adjunct to a work of this nature. In fact, as we have already hinted, Dr. Lindsay's aim throughout has been to render his book as perfect as possible, and it will not only be read with great interest and profit, but will serve as a complete book of reference on the subject of which it treats.

POPULAR GEOLOGY.*

SOME three years ago Mr. Arthur Nicols made his *début* as a writer on Geology, by the publication of a little book entitled *The Puzzle of Life*, in which he endeavoured to explain, in terms suited to the comprehension of very young students, the general teachings of Palæontology as to the nature of the animals whose existence is revealed to us by their fossilized remains, and as to the succession of life upon the earth and the arguments founded thereon with regard to the history of the earth itself. We felt bound to speak in terms of praise of this first effort of Mr. Nicols, which we suppose was a successful one, seeing that he has been encouraged to come forward again with a rather more ambitious scheme, his present *Chapters from the Physical History of the Earth* being designed as neither more nor less than a general popular treatise on Geology.

Mr. Nicols divides his book into two parts, the first of which deals with Physical Geology, the second with Palæontology. This second part appears to us to be very satisfactorily done. From its small extent it is of necessity sketchy; but the author seems to have grasped very well the conditions of the problem set before him, and we find some of the most recent palæontological discoveries referred to in their proper places. This part is arranged in historical form, that is to say, the fossils are described under the formations in which they occur. The section on Physical Geology is also fairly well done; but here, as might perhaps be expected, we meet with occasional inaccuracies which detract somewhat from the merit of the work; as at p. 17, where cleavage and bedding are confounded; at p. 28, where hornblende is mentioned as an essential constituent of granites; and at p. 33, where we are told that rocks are 'principally aggregates of sulphates, carbonates, and silicates, associated with oxygen.' The illustrations are much the same as those used in the author's former work, but with a few additions.

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* *Chapters from the Physical History of the Earth: An Introduction to Geology and Palæontology.* By Arthur Nicols, F.G.S. Small 8vo. London: C. Kegan Paul and Co., 1880.

FIELD GEOLOGY.*

A CAREFUL geological survey of a country or district is of essential importance, in order to obtain an accurate knowledge of the position, extent, and composition, of the different formations, in regard to either their scientific or their economical bearings. Aid in this direction it is partly the object of *Field Geology* to supply. The second edition of this work is considerably revised and augmented, additional matter to the extent of one hundred pages being incorporated throughout. The work consists of five parts; the first and second parts comprise an account of the necessary instruments, and their application for geological surveying, with diagrams and examples illustrative of the manner of tracing boundary lines of formations and constructing geological maps, and thus includes such instructions as are essential to the working out, either for scientific or practical purposes the geology of a district. The third, or lithological part, contains some general remarks on the character and structure of rocks and veins, notes on the more common minerals and metallic ores, their mode of occurrence, and the means by which minerals and rocks are determined; this is further usefully illustrated by a series of tables of tests for their determination in the field or in the cabinet.

The Palæontological part, by Mr. Jukes-Browne, is much improved and enlarged, and lists are given of the characteristic genera of each geological system, and of characteristic fossils of the chief sub-formations of each system.

The fifth part is devoted to the scientific and practical results of field geology, and treats of springs, streams, artesian wells, and physical features, as dependent on the geological structure of the district with which they are associated. A few typographical errors occur in the lists of fossils; the older palæozoic rocks, Cambrian and Silurian, are differently grouped by the two authors at pp. 51, 280; and the latter again does not quite accord with the remarks in the last paragraph on p. 266.

Altogether the work is considerably improved, and will form a useful field guide to those engaged in geological surveying.

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* *A Text-Book of Field Geology.* By W. Henry Penning, F.G.S. With a section on Palæontology. By A. J. Jukes-Browne, B.A., F.G.S. Second edition, revised and enlarged. Sm. 8vo. London: Baillière, Tindall, and Cox. 1879.

SCIENTIFIC SUMMARY.

ASTRONOMY.

Occultation of a Star by Jupiter.—On September 14, 1879, a very interesting phenomenon was observed at the Melbourne Observatory, the occultation of the 6½-magnitude star, 64 *Aquarii* by the planet *Jupiter*. The phenomenon was observed by Mr. Ellery, with a magnifying power of 300 on the 8-inch equatorial telescope, under very favourable circumstances, the sky being clear and the definition steady and good. Mr. Ellery describes his observations as follows (*M. N. of Roy. Astr. Soc.*, Vol. XL., page 140) :—'The smallness and faint light of the star as compared with the satellites first attracted attention. The star first appeared to touch the planet's limb at $10^h\ 5^m\ 19^s$ Melbourne mean time, and was visible in that position for nearly two minutes, when, while still making a projection on the planet's outline, it all at once appeared as if seen through a mist or haze, and *entirely projected on the planet's limb*. This faded away in about ten seconds, leaving still a decided nipple-like projection on the edge of the planet, as if the planet itself bulged out, without any signs of the true light of the star; and at $10^h\ 7^m\ 43^s{\cdot}8$ this disappeared leaving a clear outline to the disc.' The occultation was also watched by Mr. White, the first assistant, with a power of 200 on a 4½-inch equatorial reflector. The phenomenon seen agreed with that noted by Mr. Ellery, but the times were $10^h\ 6^m\ 23^s{\cdot}7$ and $10^h\ 7^m\ 40^s{\cdot}4$ respectively, the two first differing very materially from that of Mr. Ellery's, but it is very doubtful if they refer to the same phenomenon.

Mr. Turner observed the occultation with a power of 350 on the great Melbourne Reflector of four feet aperture. He states: 'At the moment of contact, the star did not instantly disappear, but seemed to possess a visible disc, the limb of *Jupiter* seeming to advance gradually upon it, the star by-and-by appearing to be bisected and then gradually disappearing altogether. The time of final disappearance was $10^h\ 7^m\ 47^s{\cdot}6$ Melbourne mean time. The time of first contact was about 35^s before final disappearance,—certainly not less; it might be more. For about 10^s after disappearance the star could be seen through *Jupiter's* atmosphere as a speck of light seen through ground glass.'

These observations are of great interest, though it is probable that the

interpretation which has been given to different stages will require material modification. The planet was moving over the star at the rate of about 0″·3 per minute, so that the final disappearance of the star was well in accord, but the time at which the planet seemed to touch the star differs very materially, being $10^h\ 5^m\ 19^s$ with Mr. Ellery, about $10^h\ 6^m\ 0^s$ with Mr. White, and $10^h\ 7^m\ 0^s$ with Mr. Turner. This undoubtedly arises from the spurious disc given to the star and the spurious fringe given to the limb of the planet from the diffraction effect of the telescope. What was taken for the star seen through the atmosphere of the planet, was almost certainly the spurious disc of the star seen through the larger spurious fringe of the planet. Any effect really due to the atmosphere must have been completely masked by this effect, due to the spurious irradiation disc given to all objects by diffraction in the telescope.

The occultation of a star by the planets has been frequently suggested as a method for determining the solar parallax, and were the phenomenon instantaneous it would be remarkably well suited for the purpose. Its great drawbacks are the infrequency of the phenomenon and the difficulty of observing the instant of the disappearance of the star at the bright edge of a planet. The present observation seems, however, to be rather favourable to the method, for the difference of the times of the observers from the mean, despite the very great difference in the instruments, is only $-\ 0^s{\cdot}1$, $-\ 3^s{\cdot}5$, and $+\ 3^s{\cdot}7$, corresponding to a difference in the place of the star of $-\ 0''{\cdot}0005$, $-\ 0''{\cdot}0175$ + $0''{\cdot}0185$, whilst the uncertainty in the parallax would give an uncertainty of $\pm\ 0''{\cdot}03$. This is the only modern observation of the occultation of a star by a planet, the last being one by Sir James South, nearly half a century ago.

Star Systems.—Mr. Stone, F.R.S., the Radcliffe Observer, has recently drawn the attention of astronomers to a most interesting system of stars in the Southern Hemisphere, which seem to present a remarkable case of an apparent connection between stars widely distant from one another. Astronomers are familiar with cases of double stars, which seem connected together in some manner analogous to the Earth and the Moon. But these stars are very close to one another, being only separated by a few seconds of arc. In the present case the stars form an isosceles triangle, with sides nearly 20 degrees in length and with a base of over 30 degrees. This system of stars consists of two stars ζ^1 and ζ^2 *Reticuli*, forming the apex of the triangle and scarcely as bright as the fifth magnitude; ζ *Toucani*, a fourth magnitude star at the southern base angle; and *e Eridani*, a star of between the fourth and fifth magnitude, at the northern base angle. All four stars are invisible from England. Besides the apparent motion in Right Ascension and the North Polar Distance, which is possessed by all stars, astronomers have long recognized the fact that many stars possess a real, independent motion in space, which, though much smaller than their apparent motion, is still too large for its existence to remain in doubt. In general this proper motion, as it is called, amounts to only a small fraction of a second of arc per annum; but in some few cases it amounts to considerably over a second of arc, or even to over two or three seconds of arc. In forming the Great Catalogue of Southern Stars, which has been the main work of the Royal Observatory at the Cape of Good Hope

whilst under his direction, Mr. Stone was led to examine all the cases of supposed great proper motion in the Southern Stars of the British Association Catalogue. In the greater number of cases they were found to arise from defective observations, but in some few cases they were confirmed. The most noteworthy instances were the group to which Mr. Stone has directed attention. From a careful consideration of each case, Mr. Stone arrived at the following conclusions :—

That the four stars of the group under consideration have proper motions much larger than the average proper motions of stars.

That the stars have a common proper motion of more than a second of arc.

That each star of the group is moving away from every other star of the group, by quantities which are small compared with the common proper motion of the group.

That, roughly speaking, the velocities of separation are larger, the larger the present angular separation of the stars.

From these conclusions it seems probable that all these stars are slowly moving away from one common point, so that many years back they were all very much closer to one another, and may have formed part of one common star system. With the present rate of motion of separation it must have taken these stars over three million years to have moved to their present positions from a point where they would have been close together. Mr. Stone remarks that it appears to him that such a system as the present one may have originated from a system of stars like α_1, and α_2 *Centauri*, which consists of two binary stars moving round each other, and with a large common proper motion, having by reason of that large common proper motion been brought sufficiently near to another binary double star to disturb the orbital motion of each and change the motion of each from closed to open orbits. The whole question opened by Mr. Stone is of the highest interest and deserves still further investigation, when the proper time arrives.

The Nebula in the Pleiades.—Some twenty years ago, Temple, whilst at Venice, discovered, with a 4-inch telescope, a fine, bright nebula close to the bright star *Merope* in the *Pleiades*. It was elliptical in form and covered an area of nearly a fifth of a square degree. Temple showed it to Valz and other astronomers, and it was seen by Peters with the 8-inch equatorial of the Altona Observatory. Subsequently it was looked for by other observers either without success, or else seen as a very faint, indistinct object. Even Temple, though it is true with another instrument and in another locality, describes it as being far less distinct than when first seen. Subsequently, when observing near Florence with larger instruments, Temple saw the nebula as large and as bright as ever. Prof. Schiaparelli of Milan also observed it with the fine refractor at Milan, and describes it as bright and distinct, and completely surrounding the star *Merope*, whilst outlying portions seemed to extend as far as *Electra*. Schiaparelli remarks : 'It is singular that so many persons should have examined the *Pleiades* without paying attention to this great nebula, which nevertheless is so evident an object on a clear sky.' Maxwell Hall, in Jamaica, also found the nebula very bright with a 4-inch telescope, and shows it as nearly half a square degree in area. Several astronomers came to the conclusion that the nebula was variable. Others

even doubted its real existence, and were inclined to ascribe its supposed observation to the effects of atmospheric action. Of late it has been drawn by several observers, so that its real existence cannot now be questioned. During this year it has been looked for by Mr. Common with the great 37-inch reflector at Ealing. The nebula was seen as a distinct object of considerable extent, but beyond it, and right within the *Pleiades*, were discovered two others, both long elliptical nebulæ of tolerably well-defined form. There seems reason to believe, therefore, that the entire background of the *Pleiades* is nebulous.

Supposed Changes on the Moon.—Two years ago those astronomers who take especial interest in observing the Moon were startled by the announcement by a German astronomer, Dr. Hermann J. Klein, of a remarkable change on the lunar surface, due to the sudden formation of a great, black crater over three miles in diameter. This new formation was stated to have appeared in a comparatively open region of the Moon, near the great valley rill of Hyginus. A full account of the early history of this supposed case of a real physical change on the Moon will be found in the *Popular Science Review* for April, 1879. The great interest of this announcement lies in the fact that according to the opinion generally held by astronomers, all active change on the lunar surface had long ere this died away; though, on the other hand, there have been few, if any, of these astronomers who have devoted much time to the study of the lunar surface, who have not strongly expressed their dissent from this opinion. It so happens that this region near Hyginus had been especially well studied, and had been frequently drawn, and not only in the past, but within very recent times. It was therefore at once recognized that this reported change, if confirmed, would afford a crucial confirmation of the view that real changes of great magnitude were still occurring on the lunar surface. If this were so, then at any moment the lunar surface might present us with the opportunity of studying a great volcanic eruption, covering the surface for miles with lava and filling the surrounding region with clouds of ashes. Even if no volcanic eruption were to take place, at any moment a grand landslip might occur, presenting a magnificent spectacle as some great mountain mass rolled crashing down a slope, ploughing up the surface before it and raising enormous clouds of *débris* as peak after peak fell with terrific energy into the depths below. There could be no doubt that the establishment of a single case of actual physical change on the Moon would increase manifold the charm and value of lunar observations.

In the article in the *Popular Science Review* for April, 1879, the history of this supposed new formation terminates with the close of February of the same year. At that period it was still uncertain whether any new object had really made its appearance in this portion of the lunar surface. That a number of observers had seen something in this region was unquestionable; but then others had failed to recognize anything of the kind. The only two experienced lunar observers who had seen this region under proper illumination, were Dr. Klein, its discoverer, and Prof. Julius Schmidt, the great selenographer of Athens. Little was known about the observations of the latter, and the former had only seen it properly on one night. Others, including more than one who were thoroughly familiar with the region, had

been completely baffled in their attempts to see this region by the extraordinary run of bad weather. At the epoch, therefore, of the article in the *Popular Science Review* for April, it had been found impossible to arrive at any definite conclusion.

Everything has now changed, and at the present time it is possible to arrive at a definite conclusion about the remarkable formation first observed by Dr. Klein, and named 'Hyginus N,' by the authority of the Selenographical Society. During March and April it was observed and drawn by Mr. Neison, who had paid particular attention to the delineation of this portion of the Moon, and who at once recognized it as a great, conspicuous object which was certainly not visible when he had drawn this region on many occasions between 1871 and 1876. Mr. N. E. Green also observed this formation on the same occasion as Mr. Neison, and their drawings published in the *Selenographical Journal* for June completely confirm one another. It was also well seen and drawn by Dr. Klein, and likewise by Messrs. Durrad, McCance, and Sadler, and by MM. Gaudibert, Borefoy, and Sluyaert. Later on, towards the end of the year and beginning of this, it was again seen and drawn by Messrs. Neison, Green, Capron, Common, Noble, and Durrad. Moreover, as most of these observers were astronomers practised in observing and drawing the lunar surface, their drawings were found to be perfectly accordant, and left no room for uncertainty as to the exact position of this remarkable formation. A series of micrometric measures, by Mr. Neison, fixed its place on the Moon as + 9° 5′ of selenographical latitude and + 6° 47′ of selenographical longitude.

There can be, therefore, no longer any doubt about the actual existence in this region of a great, black, crater-like formation, nearly as large and quite as conspicuous as Hyginus itself, and far too distinct for it to be possible to overlook it whilst drawing the minuter objects in the region all round it.

Now there can also be no doubt, that, on numerous occasions prior to 1876, this region was examined and drawings made by many observers, including Schröter, Lohrmann, Mädler, Gruithuisen, Kinau, Schmidt, and Neison, yet though they delineate many difficultly visible details, none drew or even saw this great black crater-like object which now stands so conspicuously in the very midst of this region. The only conclusion to be drawn seems to be that at this period it did not exist. If this be so, it must have been formed between 1876 and 1877. It is therefore the first thoroughly authenticated instance of a real physical change on the Moon. What this change really means, must be left for further observation to elucidate.

Great Comet of February.—From telegrams received from Dr. Gould of Cordoba, Buenos Ayres, and Mr. Gill, Her Majesty's Astronomer at the Cape of Good Hope, astronomers were led to expect that a great comet was moving in the Southern hemisphere towards the north, and would become visible in the second week in February. Anxious watch was kept at many observatories, but no trace of this great comet was seen. Letters subsequently received from Mr. Gill showed that the telegrams had been misunderstood, and that though a great comet was moving in the Southern skies towards the north, its path carried it in such a manner in regard to the Sun that it must remain invisible in the northern hemisphere. Astronomers who had been anxiously awaiting a chance of seeing a great comet were

once more disappointed. It is remarkable that nearly twenty years have gone by since a great comet was visible in the northern hemisphere, the only approach to one being Coggia's Comet, which was visible to the naked eye as a bright comet for several nights during the early summer of 1876.

From the description of this great Southern comet which has been received from the Cape of Good Hope and from St. Helena, it would seem to have been remarkable for the great length and brilliancy of its tail, which extended over the heavens for nearly 90°. The nucleus seems to have been inconsiderable. The comet seems to have moved in an orbit greatly inclined to the ecliptic, and to have passed its perihelion in the latter part of January, sweeping round in an orbit very close to the Sun. It carried its tail in such a manner that it lay almost parallel to the plane of our horizon, so that, though very vivid in the southern hemisphere in those latitudes, the tail must have been nearly parallel to the plane of the horizon, and so have been buried in the mists inseparable in the spring from a low altitude.

BOTANY.

The plants used in making Curare.—M. G. Planchon finds that four different species of *Strychnos* constitute the true basis of the poison known under the name of *curare* or *urari*, as made and used in four different regions of South America. In British Guiana, as Schomburgk ascertained, the principal ingredient of the curare made by the Macusis Indians, is the species described by him under the name of *Strychnos toxifera*, which, however, is associated with two other species, *S. Schomburgkii* and *S. cogens*. In the Upper-Amazons region the Pebas Indians prepare their arrow-poison from a species described by Weddell as *S. Castelnœana*, in honour of M. de Castelnau, who obtained it during his South American travels. It is usually associated with a species of *Cocculus* (*C. toxicoferus*, Wedd.). From the region of the Rio Negro, the roots, stems, and leaves of the plant used, were sent to Paris at the time of the Exposition of 1878; it proved to be an unknown species, and was described by M. Planchon under the name of *Strychnos Gubleri*. Lastly, the Roucouyenne and Trios Indians of Upper French Guiana employ a species described by M. Planchon as *Strychnos Crévauxii*, in honour of M. Crévaux, who lately brought specimens to Europe. He states that on the banks of the Parou, an affluent of the Lower Amazons, this plant bears the name of *ourari* or *urari*, although it is quite distinct from the species so named elsewhere. (*Comptes Rendus*, January 19, 1880.)

CHEMISTRY.

The Artificial Production of Manganese Bioxide.—By long-continued and constant heating of manganese nitrate in a flask, in an oil-bath or paraffin-bath at 155° to 162°, Gorgeu (*Compt. Rend.*, 1879, 796) succeeded in obtaining

crystals which in form, hardness, specific gravity, colour, streak, and chemical composition, agreed with pyrolusite. This experiment bears on the question, how has pyrolusite been produced in nature? and the author made numberless experiments with the purpose of solving this problem. He has, however, not succeeded by any other method than that given above in forming pyrolusite. While the author ventures to believe the method which has been hit upon as that employed by nature for the formation of pyrolusite, he directs attention rightly to the almost absolute purity of the natural product, and shows also that by the artificial method, even when impurities are added to the manganese nitrate, crystals of pyrolusite, which separate, are likewise pure. He concludes from this that in nature or by the artificial formation of pyrolusite the iron nitrate which is added as an impurity separates before the crystallization of the pyrolusite.

The Crystalline forms of Ferromanganese.—This interesting question has been investigated by E. Mallard. Alloys of these metals have been formed containing manganese varying in amount from 11 to 85 per cent. With quantities of manganese varying from 11 to 52 or 53 per cent, the crystalline form remains unchanged: a rhombic prism ∞ P (110) of 112° 33′ with the pinacoid ∞ P $\bar{\infty}$ (010). The pinacoid is so largely developed generally that the prism's faces are usually in the form of lateral stripes. When the manganese reaches 52 to 55 per cent the crystalline form suddenly becomes another, which in its general habit approaches small hexagonal prisms, and consists of a rhombic combination ∞ P (110) with an angle of about 120° and the pinacoid ∞ P $\bar{\infty}$ (010). (*Jahrbuch für Mineralogie*, 1879, 617.)

Vesbium and Norwegium.—In a few notes on these bodies by C. Rammelsberg, which appear in the *Berichte der deutschen chem. Gesellschaft*, he points out that Signor A. Scacchi has made a communication to the Academy of Sciences of Naples on an examination of certain green and yellow incrustations which cover the fissures in the lava of Vesuvius of the year 1631. They consist of silicates, and contain copper, lead, and a body which he considers new, and which he has designated Vesbium, from the old name of Vesuvius. The small quantities of this substance have allowed of only preliminary investigations. It appears to form a metallic acid of a red colour, giving colourless alkaline salts, which turn yellow on the addition of an acid. The silver salt is red or orange, the copper salt yellowish-green. Sulphuretted hydrogen gives a brown precipitate and a blue liquid, which is rendered brown by zinc. Before the blowpipe it communicates to microcosmic salt a yellow colour in the outer flame and a green one in the inner. Scacchi does not believe in the presence of molybdenum or vanadium, though many considerations render the latter not improbable. The memoir contains no numerical data beyond the statement that the silver salt contains 48·8 per cent of silver ($AgVO_3$=52·1 Ag). More definite are the statements made by Dr. Tallef Dalell [See *Popular Science Review*, 1879, 423], on the supposed new metal from the red nickel pyrites of Krageröe which he has called Norwegium (Ng.). He finds it white, not very ductile, with a specific gravity of 9·44; it melts at 350° and dissolves in nitric acid with a blue colour, which becomes green on dilution. On the reduction of the brownish oxide in hydrogen the proportion of oxygen was found to be 9·6 to 10·15. On the supposition that the oxide has the formula, NgO, we have Ng=150·6

to 141·6. The solutions are precipitated green by alkalies, but the precipitate dissolves in excess with a blue colour. Sulphuretted hydrogen gives a brown precipitate which does not redissolve; zinc reduces the sulphate first to a brown liquid, and, on boiling, to metal. Before the blowpipe it gives yellow glasses, which turn blue on cooling, and upon charcoal with soda there is formed a yellowish-green coating.

Cause of the Poisonous Character of Arsenic Oxides.—According to a paper on this subject by C. Binz and H. Schultz (*Ber. deutsch. chem. Gesellschaft*, xii. 2199), the poisonous character of these oxides depends on their easy conversion and re-conversion within the organism from the higher to the lower stage of oxidation, whereby the tissues are destroyed owing to the violent vibration of the atoms of oxygen. For the same reason they consider the other members of the nitrogen group are also poisonous.

Carbonic Acid Explosion in a Coal Mine.—An explosion of this kind took place on the 28th July, 1879, in a coal-mine at Rochabelle, Gard. Two sharp and quickly following detonations were heard, without flame, and caused by compressed carbonic acid. Three workmen were killed, and during the course of the day the gas spread itself throughout the whole of the workings, so that the lamps were extinguished. By the explosion which occurred at the end of a gallery, the latter was closed up for the space of nine metres with dislodged fragments. The gas which escaped is estimated to have amounted to 4596 cubic metres. The escape of gas continued after the accident happened. Delesse who reports the occurrence (*Compt. Rend.* lxxxix. 814), ascribes it to the generation of acid by the action of water on beds of pyrites, and the action of this acid on the beds of Jurassic limestone lying adjacent. Dumas finds that if 2000 cubic metres, or 4000 kilogrammes of carbonic acid were enclosed, about 8000 kilogr. of limestone and 6000 kilogr. of acid would suffice for its production. Considering the great surface which the much-fissured pyrites presents to the air, the production of such a quantity appears quite probable.

A new Silicium Hydride.—Ogier has submitted siliciuretted hydrogen to the action of the electric effluvium, and decomposed it entirely. A yellow substance is deposited in the tube, and the volume of the hydrogen increases to a constant limit. In five experiments where the volume taken was originally =1, it increased to 1·21, 1·22, 1·21, 1·29, and 1·26, from which the formation of the solid product, which must contain the whole of the silicium, may be reckoned. It must be,—

$$Si\,H_{1\cdot58} \qquad Si\,H_{1\cdot56} \qquad Si\,H_{1\cdot58} \qquad Si\,H_{1\cdot42} \qquad Si\,H_{1\cdot48}$$

or about $Si_2\,H_3$. This body corresponds with the suboxide of carbon or the crotonylene in the series of the hydro-carbons. When gently heated in air, it burns giving off sparks; when struck it takes fire, in chlorine it burns with flame; carefully heated in an atmosphere of nitrogen or hydrogen it evolves ordinary siliciuretted hydrogen, which spontaneously inflames in air. If strongly heated, it is decomposed into silicium and hydrogen. Siliciuretted hydrogen under the action of the electric effluvium conducts itself in an exactly analogous way to methane, which is resolved into acetylene and a condensible hydro-carbon, which has the odour of turpentine. The analogy is further borne out by the fixation of nitrogen, if equal volumes of siliciu-

retted hydrogen and nitrogen be mixed and exposed to electricity; a mixture is obtained which, besides containing nitrogen and hydrogen, also indicates the presence of, but little it is true, some one per cent of ammonia. The solid body on the other hand, which is formed, also fixes nitrogen, which, when it is fused with potash or soda, is expelled in the form of ammonia. Precisely similar results are obtained with methane. By heat alone the siliciuretted hydrogen is likewise decomposed, though only at 400°; and the decomposition is complete, into silicium and hydrogen. Arseniuretted hydrogen is also decomposed by the electric effluvium and $As_2 H$, analogous to $P_2 H$, is formed. (*Compt. Rend.* lxxxix. 1068.)

The Acid Reaction of Cryolite.—On account of the composition of cryolite $6\,Na\,F, Al_2 F_6$, and its slight but perceptible solubility in water, Stolba was led to expect that this mineral would exhibit an acid reaction. This view was found to be a correct one; when very small quantities of powdered cryolite were placed on moistened litmus paper it became gradually red. The author recommends this reaction as a test for mineralogical purposes. An attempt was made to determine the fluorine of the aluminium fluoride, which alone contributes to this reaction, by an acidimetric method, but without success.

Action of Potassium Permanganate on Potassium Cyanide.—This subject, already investigated by Péan de St. Gilles, and Cloëz, and Guignet, has been studied afresh by E. Baudrimont. By allowing a titrated solution of potassium permanganate to act on a titrated solution of potassium cyanide, it was found, 1. That the decoloration of the first-mentioned salt has a limit; 2. That the decoloration of the salt goes more easily with a rise of temperature and concentration of solution; 3. That the limit appears to be reached when two equivalents of potassium cyanide and five equivalents of permanganate are mixed; 4. That the decoloration takes place less rapidly when the solutions are more or less acidulated with sulphuric acid; 5. That the products of the reaction vary, if not as regards their nature, as regards their quantity. The formation of the following bodies was noticed: urea, carbonic acid, nitric acid, nitrous acid, oxalic acid, formic acid, and ammonia, due to the decomposition of the urea. The formation of these substances is in accordance with the subjoined formulæ:

$$2\,C_2\,N\,K + Mn_2\,O_7, KO + 6\,HO = C_2\,H_4\,N_2\,O_2 + C_2\,O_4, (KO)_2 + KO, HO + Mn_2\,O_3\,HO$$

$$C_2\,N\,K + 2\,(Mn_2\,O_7, KO) + 2\,HO = NO_3, KO + C_2\,O_4, (KO)_2 + 2\,(Mn_2\,O_3, HO)$$

$$2\,C_2\,N\,K + 5\,(Mn_2\,O_7, KO) + 6\,HO = 2\,(NO_5, KO) + 2\,(C_2\,O_4, KO)_2 + KO\,HO + 5\,(Mn_2\,O_3, HO)$$

Formic and oxalic acids may be regarded as derivatives of hydrocyanic acid and cyanogen which are the nitril of formate and oxalate of ammonia respectively; their formation is simply explained by the action of water on the cyanogen molecule. As regards the ammonia, it appears to be formed by a secondary action of free potash on urea after the complete decomposition of the permanganate. (*Comp. Rend.* lxxxix. 115.)

GEOLOGY AND PALÆONTOLOGY.

The Eozoon Question.—The reader is, doubtless, familiar with the fact that the question of the animality of *Eozoon canadense* has been for a considerable time a matter of lively discussion. Dr. Dawson, its original describer, Dr. Carpenter and others, have stoutly maintained the organic nature of this supposed earliest evidence of the existence of life upon the earth; while MM. King and Rowney have just as persistently argued on the other side; and within the last year or two Dr. Otto Hahn, of Reutlingen, and Prof. Möbius, have strenuously maintained that there is nothing in the so-called *Eozoon* which cannot be referred to mineral structure.

The most elaborate work in opposition to the organic nature of *Eozoon* is the great memoir of Prof. Möbius, which appeared about a year ago in the *Paläontographica;* and this, from its fulness of detail and the magnificent way in which it is illustrated, must be always of value, whatever the ultimate verdict in the case of *Eozoon* may be.

The latest contribution to this subject is by Dr. Hahn, above mentioned, who has already published two memoirs in support of the purely mineral nature of *Eozoon* in the *Jahreshefte* of the Württemberg Natural History Society These were serious productions, but his last production we cannot help regarding as an enormous joke, although it is difficult to understand any one taking the trouble to produce a pamphlet of seventy-one pages, illustrated with twenty-nine plates, merely for the purpose of poking fun at a scientific opponent. The book has, indeed, been treated as serious by several writers, both in Germany and in this country; but it seems to us to be ironical throughout; and every one knows that of all figures of speech irony is the one which is least readily understood. The very title sounds like a joke; it is, when interpreted, 'The primordial cell, with the proof that granite, gneiss, serpentine, mica, certain sandstones, also basalt, and finally meteoric stones, and meteoric iron, consist of plants.'*

The author seems to have been moved to the production of this extraordinary work by the pertinacity with which Dr. Dawson maintains the animality of *Eozoon*, and partly by the indications given by him in recent publications of what he regards as possible traces of other organisms in the Laurentian rocks in which *Eozoon* occurs.

Having visited Canada, and obtained specimens of the so-called *Eozoonal* limestone, Dr. Hahn professes to find himself converted from his former scepticism; he admits that the so-called *Eozoon* is a fossil, but declares it to be a plant, and not an animal, and accordingly re-christens it *Eophyllum canadense*. Of course the name *Eophyton*, which would naturally suggest itself for the 'dawn-plant,' is preoccupied; but the absurdity of using *Eophyllum* as the generic name of a plant without any leaves is an additional evidence of the ironical character of the book.

Having once got this clue to the mystery the author of course goes on

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* *Die Urzelle, nebst dem Beweis dass Granit, Gneiss, Serpentin, Talk, gewisse Sandsteine, auch Basalt, endlich Meteorstein und Meteoreisen aus Pflanzen bestehen.*

finding similar manifestations of his 'Urzelle' in all sorts of rocks, belonging to all sorts of series, in granite, in Carrara marble, in various crystalline rocks, in basalt, and finally in meteoric stones and meteoric iron. Not unnaturally the forms of the plants are numerous and diversified, and the author distinguishes a host of genera and species, in the names of most of which we find traces of the same carelessness as in the case of *Eophyllum*. These simple cellular Algæ are described as presenting points of structure which belong to the most dissimilar groups of plants; they have stems, buds, and prothallia, the stems and cells are seen in conjugation, &c.

We may cite a few of the names conferred upon these wonderful organisms. One is named *Ophthalmia Hochstetteri*, another *Dufferinia*, in honour of Lord Dufferin, and another *Victoria*, in honour (?) of the Queen of England; Mr. Darwin is complimented by being made to stand sponsor to a *Marmora Darwini*, the Crown Prince of Austria has a *Stygia* named after him; while the German Emperor is honoured by having his name attached to a species coming from above instead of below, namely, *Urania Guilielmi*, discovered in the meteoric storm of Knyahinya. Two people upon whom this questionable compliment is conferred come off rather worse than the rest, and one of these is no other than Prince Bismarck, 'unser Reichskanzler.' 'The first animal' is named *Titanus Bismarki*, in honour of this distinguished individual; it is said to be a serpuliform worm, and its resemblance to the illustrious chancellor of the German Empire would appear to consist in its wearing a cuirass (*Kieselpanzer*) and being of a rather paunchy figure. It is to be hoped that Prince Bismarck can enjoy a joke even at his own expense, otherwise Dr. Hahn may find cause to repent him of this one. The other name is worse, and while its presence here seems to be conclusive as to the ironical character of this book, its use is, we think, most strongly to be reprobated. Dr. Hahn names one of his species, obtained from flint-nodules which he erroneously supposed to be from the Silurian of Canada, *Photophoba Dawsoni*, 'Dawson's Light-dreader,' we presume, or it might be 'Dawson's dread of the light;' and as if this was not a sufficient affront, he expresses a hope that over this delectable piece of false wit he and Dr. Dawson may shake hands in token of reconciliation!

But we have not yet quite done with Dr. Hahn. The essence of an ironical work, such as we believe this to be, is to start with absurd propositions and work them out as logically as possible to their extreme consequences; and thus, out of the discovery of *Eophyllum canadense*, there springs under the author's pen a new theory of rocks, and indeed of the formation of the earth, and all that it contains. These cellular plants that we have been discussing are by no means to be regarded as fossils in the ordinary sense of the word; they are, in most cases, the actual producers of the rocks in which their traces occur; even the crystalline rocks not being exempt. 'Granite,' we are told, 'is nothing but a plant-mass; there is no rock-mass with it; all is plant!' The crystals of minerals themselves seem to represent the cells of the plant! Hence we are led by degrees to the recognition of the startling fact that the crust of the earth on which we live is a sort of Sargasso bed, built up in the course of ages by the energy of the 'Urzelle,' and floating upon some kind of primordial ocean. The 'Urzelle' itself is proved to be still at work by its occurrence in basalts and lavas of

recent geological date; hence its growth does not require the presence of light and air, but only of moisture and heat, and we have here a remarkable coincidence with the eruptive phenomena which have brought the later manifestations of these marvellous organisms to the surface, and with the known distribution of volcanoes at no great distance from the sea!

We have thought it worth while to give a sketch of the general contents of this book, both because it has been regarded as written with a serious intention by critics who have blamed the author for burthening science with such a mass of absurdly useless names, and because it is in itself a curious and noteworthy phenomenon. Its style is compared by one of its German critics to that of the Book of Revelations, and it must be confessed that there is some similarity. Had the author written one-third of the quantity, and illustrated his pamphlet with only two or three plates, it would have told much more powerfully against his opponents. Dr. Hahn is not quite a Swift, and even the great Dean's irony was not appreciated by everybody.

We understand that Professor King has a work in preparation which will bear upon this subject; it will be entitled 'An Old Chapter of the Geological Record with a new Interpretation: or Rock-metamorphism, especially Methylosis, and its resultant imitations of Organisms. With an Introduction giving a history of the controversy on the so-called *Eozoon Canadense*.'

Archæopteryx macrura.—At the meeting of the Swiss Society of Natural Sciences, held at St. Gall in August, 1879, Professor Carl Vogt read a most interesting communication upon the second specimen of this remarkable fossil organism, which was found in the lithographic slate of Pappenheim some years since, but of which no naturalist had hitherto been able to make a careful examination.

The genus *Archæopteryx* was established in 1861 by Hermann von Meyer, upon the evidence of a feather found fossil in the lithographic stone of Solenhofen. The species was named *Archæopteryx lithographica*, in allusion to the formation in which it was found. Somewhat later a much more important specimen was found by a M. Häberlein in the same deposits. This consisted of a slab of limestone containing various parts of a feather-bearing creature, including limb-bones, vertebræ, traces of the skull, and especially a long, slender tail, from each vertebra of which sprang a pair of quill-feathers. This slab was subsequently purchased for the British Museum, and in 1863 Prof. Owen described it as a species of Von Meyer's genus under the name of *Archæopteryx macrura*, in allusion to the length of the tail. Notwithstanding this remarkable peculiarity, *Archæopteryx* was referred to the class of birds, but placed in a special group, to which the name of *Saururæ* was given. The animal has always excited the greatest interest, from its apparent combination of reptilian and avian characters, a circumstance which acquires the more importance from the occurrence of the fossils in beds which are so rich in Pterodactyles. It seemed that we had here in the same deposit the most bird-like of Reptiles and the most reptile-like of Birds.

The second specimen, discovered by M. Häberlein's son in the lithographic stone of Pappenheim, is now, we believe, in the Senckenbergian Museum at Frankfort; but for some years after its discovery no one was allowed to see

it. According to Prof. Vogt's account of it, it is nearly complete, and its wings are unfolded as if in flight. The head is small. Implanted in the upper jaw two small conical teeth may be detected with a lens. Eight cylindrical cervical vertebræ, furnished with very fine ribs directed backwards, were counted. The dorsal vertebræ appear to be ten in number, thick and short, and destitute of spinous processes. Their ribs are slender, curved, and pointed at the end; they show no flattening, nor are there any traces of the uncinate processes which in most birds spring from the posterior margin of each rib and rest against the succeeding one. There are also very fine sternal ribs, which appear to be attached to a linear abdominal sternum. The pelvis is still enclosed in the matrix. The tail is very long, and agrees with that of the first known specimen. The hind limbs are hardly so perfect as in Prof. Owen's example, but they show with certainty that the fibula is completely united to the tibia, and only marked off by a not very strong longitudinal furrow. The anterior limbs, on the contrary, are more perfect than in the original example. M. Vogt thinks that two scapulæ are recognizable, and that there is no bone representing the furculum. The two coracoids seem to be in contact in the median line, and the sternum is reduced to zero. The bones of the arm present no features peculiar to reptiles or to birds; they have been already well described by Prof. Owen. But the bones of the hand in the new specimen show characters not previously known. The carpus shows only one small globular bone. The digits are well preserved in both limbs; and from the information now obtained it appears that the hand of the *Archæopteryx* cannot be compared to that of a bird, or to that of a pterodactyle, but only to that of a tridactyle lizard. There are three long, slender digits, armed with curved and sharp-edged claws. The thumb is the shortest, and is composed of a short metacarpal, a long phalange, and the ungual phalange. The other two digits consist of a metacarpal and of three phalanges. The wing-feathers were attached to the cubital margin of the fore-arm and hand, but no special adaptation of the skeleton to this purpose can be observed The thumb was free, like the other two digits, and did not support a winglet. Thus, if the feathers had not been preserved no one could have suspected from the examination of the skeleton that the animal had been furnished with wings.

Prof. Vogt sums up the information we now possess as to the organization of *Archæopteryx*, with special reference to the question of its systematic position. The head, neck, thorax and ribs, tail, shoulder girdle, and the whole anterior limb, are clearly constructed as in Reptiles; the pelvis has probably more relation to that of Reptiles than to that of Birds; the posterior limb is that of a Bird. The reptilian homologies certainly preponderate in the skeleton. There remain the feathers; these are unmistakably birds' feathers, with a median rhachis and with perfectly formed barbules. The new slab shows all the feathers in their place. The remiges, as already stated, are attached to the cubital margin of the arm and hand; they are covered for almost half their length with a fine filiform down; none of them project beyond the others, and the wing is rounded in outline, like that of a common fowl. There are thought to be indications at the base of the neck of a collar like that of the condor. The tibia was covered throughout with feathers, so that the leg of the *Archæopteryx* must have presented an external

resemblance to that of a falcon, as also, according to Prof. Owen, in its anatomical structure. Each caudal vertebra bore a pair of rectrices, but all the rest of the body was apparently destitute of feathers, otherwise traces of them must have been found upon a slab which has preserved even the smallest details of a fine down.

Prof. Vogt regards *Archæopteryx* as neither a reptile nor a bird. It constitutes according to him an intermediate type of the most strongly marked description, confirming the views of Prof. Huxley, who has united reptiles and birds, under the name of Sauropsida, as forming a single great section of the Vertebrata. It is one of the most important signposts on the road which has been followed by the class of birds in its gradual differentiation from the reptiles from which it originated. A bird by its integument and hinder limbs, it is a reptile in all the rest of its structure; and thus its conformation can only be understood if we accept the evolution of birds by progressive development from certain types of reptiles. The Cretaceous birds described by Prof. Marsh indicate a later step in the same direction.

In discussing the stages by which *Archæopteryx* passed to arrive at the form under which we knew it, and the mode in which adaptation for flight has acted upon the different parts of the body, Prof. Vogt shows, in the first place, that this adaptation in Vertebrata is by no means necessarily combined with that of an upright position. This is seen in the Pterosauria and the Bats. The structure of the hind feet that occurs in the Dinosauria, the *Archæopteryx*, and in birds, is therefore, he thinks, independent of the faculty of flight, and only stands in relation to the possibility of sustaining the body upon the hinder feet alone; and hence the resemblance in this respect between Dinosauria and birds by no means indicates real affinity. At the utmost he would regard it as possibly indicating a genetic connexion between the Dinosauria and the Struthious birds, but this would involve a multiple origin of the class of birds.

The search in deposits older than the Upper Jurassic for reptiles which may be related to *Archæopteryx*, and thus indicate earlier stages in the process of evolution, would seem to be vain, since the fossils we possess are destitute of tegumentary parts, and it is very difficult to say *à priori* with what cutaneous structures these creatures were covered. At the same time, as Prof. Vogt points out, there is complete homology between the scales or spines of reptiles and the feathers of birds. The reptilian structures, he remarks, differ in no respect from the wart-like stumps which appear in the embryo bird as the first traces of plumage; the feather of the bird is only a reptile's scale further developed; and the reptile's scale is a feather which has remained in the embryonic condition. The feathers of *Archæopteryx*, which are so perfect, must undoubtedly have been preceded in some pre-existing reptiles by cutaneous structures representing persistently the different stages of the embryonic development of the feathers. We may, therefore, imagine the ancestors of the *Archæopteryx* as lizard-like, terrestrial reptiles, having feet with five, hooked, free digits, showing no modification of the skeleton, but having the skin furnished at different points with elongated warts, downy plumes, and rudimentary feathers, not yet fitted for flight, but susceptible of further development in the course of generations.—(*Bibl. Univ.*, December 15, 1879; *Ann. and Mag. Nat. Hist.*, February, 1880.)

Correlation of the Rocks of the South of Ireland with those of other Districts.—Prof. Hull, Director of the Geological Survey of Ireland, read an interesting paper before the Geological Society on this subject, on March 10th. He referred to a previous paper in which he had discussed the geological age of the Glengariff (or Dingle) beds, and arrived at the conclusion that these are Upper Silurian (Upper Ludlow) deposits, greatly expanded vertically. These rocks were disturbed, elevated, and denuded, and ultimately submerged in such a manner, that the later Old Red Sandstone and Lower Carboniferous strata are found resting transgressively upon them. In comparing these Irish rocks with those of North Devon, Prof. Hull first showed that the true division between the Old Red Sandstone and Carboniferous series must be taken at the top of the 'Kiltorcan Beds' with the freshwater bivalve *Anodonta Jukesii*. He held that the Coomhola grits and Carboniferous slate series of the south of Ireland are represented in Devonshire by the Barnstaple, Pilton, and Marwood beds, thus relegating these divisions of the Devonshire geologists to the Carboniferous. This conclusion is borne out by palæontological evidence.

The Upcot Flags and Pickwell Down Sandstone are to be regarded as the representatives of the Kiltorcan beds and Old Red Sandstone and Conglomerate of the south of Ireland; but here the correlation of the formations of the two districts ends, for the Martinhoe, Ilfracombe, Hangman, and Lynton beds have no representatives in the latter locality. These groups, the Lower and Middle Devonian marine deposits, were deposited during the upheaval of the Irish Upper Silurians.

The Foreland Grits, which lie at the base of the whole Devonian series in Devonshire being identified by him with the uppermost Silurian, or 'passage-beds' of Murchison, the author inferred that the great gap existing in Ireland between the Glengariff beds and the succeeding Old Red and Carboniferous series, was filled up in Devonshire by the beds lying between the Pickwell Down Sandstone and the Foreland Grits.

Accepting Prof. Geikie's suggestion, that the Scotch 'Lower Old Red' is the representative of the Irish Glengariff beds, Prof. Hull concluded that this 'Lower Old Red' is really the lacustrine equivalent in time of the marine uppermost Silurian strata (a view which is supported by fossil evidence); while the 'Upper Old Red' of Scotland is the equivalent of the formation bearing the same name in Ireland. Hence it follows that there is only one formation properly called 'Old Red Sandstone,' and this is the equivalent of the Upper Devonian as restricted to the Pickwell Down Sandstone of Devonshire.

In the district north of the Severn, embracing the region of 'Siluria,' the limestone shale, both by its position and fossils, is shown to be the greatly reduced representative of the Barnstaple, Pilton, and Marwood beds; while the Yellow and Red Sandstone and Conglomerate represent the Pickwell Down Sandstone, or true Old Red Sandstone. Between this and the Upper Ludlow and Passage-beds comes the Cornstone group, to which Mr. Godwin-Austen and Prof. Ramsay have assigned a lacustrine origin. Prof. Hull is of opinion that the thickness of this group has been greatly overestimated, and regards 5000—6000 feet as the maximum. It was probably deposited in an estuary opening seawards towards the south. He accepted

the conclusion, suggested by M. Dewalque, that the Cornstone group represents the Middle and Lower Devonian groups of Devonshire, lying between the Old Red Conglomerate and the Upper Silurian. The Belgian 'Psammites du Condroz,' a great group of sandstones lying between the Carboniferous Limestone and the 'Calcaire du Frasne,' is to be regarded as the representative of the Pickwell Down Sandstone, and therefore of the true Old Red, notwithstanding the presence in it of marine fossils.

The principal conclusions to be drawn from these arguments are as follows: There is only one Old Red Sandstone properly so called, represented in Devonshire by the Pickwell Down Sandstone, and in Scotland and Ireland by the so-called Upper Old Red, in which case the Old Red Sandstone is not the equivalent of the marine Devonian strata, but surmounts them. Secondly, at the close of the Upper Silurian period, all the western and northern portions of the British Isles were disturbed and elevated into land surfaces, while subsidence and deposition of marine strata were taking place over the south of England and Wales, and adjoining parts of western Europe, the Lower and Middle Devonian groups being then formed. At the commencement of the Upper Devonian stage there was a general subsidence; lakes were formed over the Irish and Scottish areas, on the shelving shores of which the Old Red Conglomerate was accumulated; and at the commencement of the Carboniferous period a further subsidence took place, bringing in the waters of the ocean over all the submerged areas.

MINERALOGY.

Hedyphane containing baryta from Laangban.—Lindström describes (*Jahrbuch für Mineralogie*, 1879, 896) this specimen as white to pale yellow-white, and except as regards its showing the barium reaction and higher specific gravity of 5·82, according in every respect with hedyphane. The analyses given below show, under I., the composition as found, and, under II., the insoluble portion subtracted as well as the lime carbonate and a quantity of oxygen equivalent to the chlorine.

	I.	II.
Arsenic acid	28·18	29·01
Phosphoric acid	0·53	0·55
Carbonic acid	1·07	Lead 9·19
Chlorine	3·05	3·14
Lead oxide	49·44	41·01
Baryta	8·03	8·27
Lime	8·99	7·85
Magnesia	0·24	0·25
Iron oxide	0·08	0·08
Soda	0·15	0·15
Potash	0·09	0·09
Insoluble residue	0·42	—
	100·27	99·57

Excluding the iron oxide, potash and soda we have the general formula,

$$3\,(3\,RO,\,As_2O_5) + Pb\,Cl_2$$

Herrengrundite, a new basic Copper sulphate.—This body is described by Brezina as occurring in little plates, 1 to 2·5 mm. in diameter, scarcely 0·2 mm. in thickness, and having apparently the composition indicated by the numbers—

Sulphuric acid	23·04
Copper oxide	57·52
Water	19·44
	100·00

2·05 lime found in the analysis have been subtracted, as well as the requisite amount of sulphuric acid and water to form gypsum with it. The colour of the plate-like crystals is dark emerald-green, the hardness, 2·5: it is almost brittle, the cleavage along, oP (001) complete. The axes are,

$$a : b : c = 1{\cdot}8161 : 1 : 2{\cdot}8004$$

Titanomorphite. (*Jahrbuch für Mineralogie*, 1879, 897.)—Von Lasaulx directs attention to the widely-spread and characteristic product of the decomposition of menaccanite, which occurs in rocks, and which he has always held to be a lime titanate analogous to perowskite in point of composition. He has recently met with it in larger quantities. In a slaty amphibole rock from the gneiss of Lampersdorf, near Reichenbach, in Silesia, occur rounded nodules of rutile which are covered with a greenish or yellowish-white zone of a granulated or fibrous product of decomposition. Sufficient of this material was collected for analysis, and found to consist of

Lime	25·27
Titanic acid	74·32
Iron protoxide	Trace
	99·59

These numbers correspond with the formula,—

$$Ca\,O_1\,2\,Ti\,O_2$$

which is that of the new mineral. (*Jahrb. für Mineralogie*, 1879, 569.)

A new Barytic Felspar.—Des Cloizeaux found in the collection of the *Muséum d'Histoire Naturelle* a few fragments of a felspar prism, of unknown locality, which are transparent or only translucent, and resemble the albite of St. Vincenz, Styria. The two cleavage directions, oP (001) and $\infty P \overline{\infty}$ (010) form an angle of 86° 37′, and the reentering angle of the base was found to be 173° 14′. These values approach closely those found for labradorite, while the optical characters of the felspar in question more closely agree with those of oligoclase and albite. To decide this question, the mineral was analysed, and the following results arrived at:

Silicic acid	55·10	8
Alumina	23·20	3
Iron peroxide	0·45	
Baryta	7·30	1
Lime	1·83	
Magnesia	0·56	
Soda	7·45	
Potash	0·83	
Loss by heating	3·72	
	100·44	

While the analysis agrees with that of a barytic andesine, the mineral shows by its optical properties and the angle of its cleavage planes, that it is not that; in fact, it agrees with none of the felspars which have yet been examined. (*Jahrbuch für Mineralogie*, 1879, 591.)

PHYSICS.

The Thermo-electric behaviour of Aqueous Solutions with Mercurial Electrodes, has been communicated to the Royal Society by Mr. G. Gore. He used for this purpose an apparatus consisting of two thin glass basins, containing each a layer of mercury about one centim. deep, covered by a layer three centim. deep, of the aqueous solution to be examined. A bent glass tube of the shape of an inverted U with a vertical canal at the highest point was filled with the liquid, and inverted over the mercury; two stout glass tubes with a platinum wire sealed into their lower ends, one filled with mercury, were placed in the mercury of the basins before adding the liquid. Terminals of platinum touched the mercury, but not the liquid. Thermometers were immersed in either pool of mercury. One basin was supported on wire gauze with a Bunsen's burner beneath it, the other on a shelf at the ordinary temperature. The mercury was carefully purified. A galvanometer was used to test the completeness of the circuit. One basin was then heated, and the deflection of the galvanometer noted at various temperatures. Solutions were chosen which had little chemical action on mercury, and which were free from any visible film or dulness over the heated metal. The galvanometer was astatic with two coils, each of 50 ohms resistance, connected together. A table was obtained with the solution at the top, in which the hot mercury was most positive at 180° Fahr., and that at the bottom in which it was most negative. Far at the head stands phosphate of ammonium, with a deflection of + 28° followed by carbonate of sodium with + 19·0. At the other extremity of the scale stands cyanide of potassium, which sinks from − 14°·0 in a solution of 5 grs. to − 50°·0 in one of 100 grs. In order to ascertain whether this order agreed with that of a series arranged according to the different degrees of electromotive force of the various couples, two similar basins were charged with the two solutions and connected with a differential galvanometer with currents in opposite directions, to ascertain which gave the strongest currents.

On examining the results it is observed that the effects are not manifestly related to the chemical nature of the solutions. It is difficult to prove how far chemical action occurred as a cause of currents, though it cannot have been great. The strength of the solution clearly affected the quantity, and in some instances the direction of the current. Stirring, and previous heating of the solution, also influenced the deflection. It appeared that the currents were due to heat acting on and altering particular molecular structures, and that the difference of electromotive power was not due to chemical variety, but to differences of molecular arrangements.

Measurements by means of Thomson's Galvanometers are not, according to M. Gaiffe, as reported in the *Comptes Rendus* for January, found to be proportional to the value of the currents measured, but to be exaggerated as those values increase. This error arises from the fact that the angles of

deflection of the magnet are doubled by the reflection of the mirror, and that consequently it is not the tangents of the real angles of deflection that one reads on the scale, but the tangents of the doubled angles. This assumption may indeed be made with very small angles, but even with a deflection of 8° the indications are already sensibly exaggerated at the ends of the scale. A bifilar suspension with two silk threads very close together reduced the erro s to less than one hundredth of the value measured.

Distribution of power by Electricity has formed the subject of experiments by M. Tresca, using a Gramme machine making 1200 revolutions per minute. The current was transmitted by copper-wires to carts at different distances, two Gramme machines being placed on the carts, and acting on a windlass which drove a double Brabant plough so as to make a furrow 220 metres long. The velocity of the plough when the circuit was 800 metres, was 88 centimetres per second, the shaft making 1123 turns per minute; when the circuit was increased to 1300 metres the velocity was 70 centimetres, and the revolutions of the shaft 890. The effective work was estimated at three-horse power.

Electrical Storage has been described by Professors Houston and Elihu Thomson in the *Journal of the Franklin Institute*. Such a process has long been suggested, but has failed to become of practical use, (1) from cumbrousness, owing to the large conducting surface required; (2) from loss of energy due to evolution of gas during charging; (3) lack of constancy; (4) limited capacity. These defects apply to Plante's secondary battery; the system they suggest consists of a saturated solution of zinc sulphate, at the bottom of which is placed a plate of copper connected to an insulated wire. At the top is placed a second copper plate, one of hard carbon, or of some metal unchanged by contact with zinc sulphate solution, and less positive than metallic zinc. Charging of the cell so constructed is effected by passing a current through it from the lower to the upper plate. The current employed is that from a dynamo-electric machine, and the result is the deposition of metallic zinc on the upper plate, and the formation of a dense solution of copper sulphate overlying the lower. The cell, in fact, becomes a gravity battery, acting as such, till all the deposit is redissolved.

A somewhat similar plan has been employed by Mr. C. F. Varley to distribute time-signals, but has been superseded by Leclanché cells of large dimensions. It consisted of gutta-percha chambers $4\frac{1}{2}$ in. high, each with a division of $1\frac{1}{4}$ in. deep in the centre; the shallow cells thus formed being half filled with mercury, and connected with the mercury in the corresponding cell of the neighbouring chamber. The last carbons were connected to terminals and to the discharging wires; the cells were filled with zinc sulphate and sulphuric acid. On passing a current through sixty such cells from 150 small Daniels, amalgamated zinc was formed on one side and bisulphate of mercury on the other. The arrangement had a very low resistance.

Cast-Iron Magnets are being made by M. Carré by running a soft and slightly carburetted metal in earthen crucibles. Just before pouring, 10 to 15 per cent of steel-filings and about 1 or 1·5 of nickel with ·25 per cent of copper; or 2 per cent of tin and 0·5 per cent of copper, are added. It can then stand tempering at a cherry-red heat.

Nature extracts from the *New York Nation* a marvellous account of

an instrument which its inventor, Prof. Alfred W. Mayer, terms the 'Topophone,' or Sound-place. 'It consists of a vertical rod passing through the roof of the deck cabin, bearing on its upper end a horizontal bar, carrying two adjustable resonators, below which a pointer is set at right angles with the bar. Rubber tubes from the resonators pass through the roof of the cabin, and unite in a single pipe connected with a pair of ear-tubes. The vertical rod is turned, by means of a handle, in any direction. The first step is to tune the resonators accurately to the pitch of the sound under observation, and the second to fix them 'at a distance from each other somewhat less than the length of wave of that sound; next, by turning the handle to bring them simultaneously on the wave-surface, when, as they both receive at the same instant the same phase of vibration on the planes of their mouths, it will result that, if the connecting tubes be of the same length, the sound-pulses acting together will be reinforced to the ear; but if the tubes differ in length by one half the wave-length of the sound, the pulses will oppose and neutralize each other. At this moment the horizontal bar is a chord in the spherical wave-surface, of which the distant fog-horn is the centre, and the pointer represents a radius directed to the place from which the sound emanates.' This seems a pretty and useful little problem to set to a skipper in a gale and a fog, with an unknown danger-signal hardly audible in the offing. It is just possible that he might be better employed on deck.

Dynamo-electric Machines for Telegraphic Purposes, are described in a late number of the *Scientific American*. They are intended to replace 14,300 gravity battery elements, and 4600 bichromate of potash cells. The machines are on Siemens's system, connected in series with their field-magnets excited by a current from a single Siemens machine. One commutator-brush of a machine is connected with the brush of opposite polarity in the next, and so on, so that a current of any desired potential may be had from the different machines in the series. The E. M. F. in the first being 50 volts, the second will be 100, and the third 150. A patent to this effect was taken out by Mr. H. Wylde in 1878 in this country. Dr. Schwendler, Electrician to the Indian Government, finds the dynamo-electric current better for telegraphic purposes than those from batteries. A signal current can be obtained from that maintaining a powerful light by derivation without perceptibly diminishing the lighting power.

Atmospheric Polarization and the influence of Terrestrial Magnetism on the Atmosphere form the subject of a memoir in the *Annales de Chimie et de Physique*, by Henri Becquerel. First observed by Arago in 1809, it was further worked out by Babinet, Brewster, Bernard, Liais and Rubenson. If the light emitted by the atmosphere in the sun's vertical plane be analysed at an hour when the luminary is at a small elevation above the horizon, it is found to be faintly polarized in his neighbourhood, the polarization increasing towards the zenith. The maximum occurs at an angle of 90° from the sun, falling again to zero at a point named Arago's neutral point. Up to this the plane of polarization is vertical, below it it is horizontal. At sunrise and sunset, the neutral point is from 20° to 30° above the horizon. Babinet found a second above the sun, and Brewster a third at about the same distance below. There seem to be other secondary neutral points related to those above named, and discoverable in certain states of the atmosphere.

The explanation has generally been referred to reflection of the solar rays for the vertical, and to refraction joined to secondary reflections, for the horizontal polarization. The neutral points thus become spots where the vertical and horizontal actions have equal and opposite intensity. Hypotheses regarding the method of reflection have been founded on the partial opacity of the air itself, on that of suspended solid particles, and on small vesicles of water. Hagenbach referred it to layers of air of unequal density. Tyndall considers it not due so much to simple reflection as to a peculiar phenomenon which attends on luminous gas.

Wheatstone, by means of an apparatus which he termed a Polar Clock, showed that by pointing this to the pole the position of the plane of polarization would approximately give solar time. M. Becquerel set himself to determine with precision the relative positions of the sun and of the plane of polarization.

The apparatus he devised for these researches consists of a Savart polariscope mounted in a divided circle, and observed by means of a total reflection prism. It can be turned in any direction, azimuth and altitude circles giving independently the co-ordinates of the point observed with reference to the magnetic north.

The observations consisted in determining on the same divided circle the position of the plane of polarization, and the shadow of the optical axis of the apparatus thrown by the sun itself. Cross wires were fixed to the end of the movable tube which threw an image on a plane surface attached to it. These were afterwards replaced by two needle-points which threw their shadow on a small screen regulated to move in the sun's plane. The Savart polariscope was found more delicate than others depending on equality of tint in the presence of much diffused non-polarized light; but instead of observing the greatest intensity of the fringes, their disappearance at an angle of 45° was noted, which proved more susceptible of accurate determination.

The process of observing consisted in taking three double measurements of the solar plane, with the time. Then a series of observations of the plane of atmospheric polarization followed, and then again a repetition of the first measurements. These were plotted out with lines as Abscissæ and the numbers found as Ordinates. The former proved the more accurate. Several of them are reproduced in the memoir.

The results, stated briefly, are as follows: 1. The plane of polarization from any given point in the sky does not generally pass through the sun, but usually below it. At the zenith, the planes of the sun and of polarization coincide. At the pole their angle is small, and was hence unnoticed by Wheatstone. They increase towards the horizon. As a rule the polarized light coming from the sky behaves like a luminous ray starting from the neighbourhood of the sun and reflected towards the observer; but it contains rays polarized by refraction also, of variable intensity, in some cases sufficient to annul the opposite effect, and to produce 'neutral points.' Indeed the sun is not to be regarded as the only source of atmospheric illumination, but also the air and the earth acting as reflectors. Each of these may displace the plane.

As regards the action of terrestrial magnetism, it appears that the plane of polarization undergoes rotation in a direction always the same, direct if

observed northwards, inverse if southwards. It is in the same direction as a current would take to produce terrestrial magnetism. The rotation, if due to magnetism, should not be detected in a line perpendicular to the dip needle: by experiment this is found to be the case.

The author of the memoir has already contributed valuable researches to the *Comptes Rendus* of the Academy on measuring the rotatory power of certain gases, especially of air. These have been noted in this Summary; and so has the observation of the power of terrestrial magnetism in rotating a ray passing through carbon bisulphide. By combining these with the facts above stated, M. Becquerel shows in conclusion, (1.) The existence of a variable angle between the sun's plane and that of atmospheric polarization for the same point; (2.) The periodic diurnal variation of this angle, closely connected with change of illumination; (3.) The evidence of a magnetic influence of the earth on the atmosphere, producing a small but definite deviation in the plane of polarization.

The History of Musical Pitch was given in detail by Mr. Alex. J. Ellis before the Society of Arts on March 3rd, the paper beng in some sense supplementary to a former communication on May 22nd, 1877, to which a Society's silver medal was awarded. It had been objected to the former paper that the instrument used for measuring pitch, namely, Appunn's Reed Tonometer, was hardly trustworthy, and that the results thus obtained were probably incorrect. M. Randolph Kœnig especially wrote in this sense to the *Monde* of June 19th, 1877. 'A strange and unexpected attack,' he writes, 'has been made in England on the accuracy of the French official Diapason. Mr. Ellis, having found that the notes of a Tonometer composed of sixty-five harmonium reeds did not agree with it, has thought fit to declare before the Society of Arts that the normal A does not give 870 simple vibrations but 878. Mr. Ellis having, moreover, found that tuning-forks of my manufacture were accurately in tune with the French A, did not hesitate to affirm that all these instruments, including my large Tonometer, were necessarily false.' He then quoted a letter from Helmholtz showing the inaccuracy of Appunn's standard, and intimates, with some warmth, that Mr. Ellis had neglected to verify his test instrument before using it. The writer of the present notice had already drawn the attention of the Musical Association to the same defect at a meeting on Nov. 6th, 1876. It was afterwards shown by Lord Rayleigh that the error was due to the natural influence of the reeds on one another when in a state of vigorous vibration. Mr. Ellis, in the present paper, frankly admits the charge, saying, 'What the cause of this "drawing" of the beats may be has not yet been investigated.' (This is incorrect.) 'Its direction and amount was, I believe, entirely unknown previously. I feel that I owe an apology to Herr Kœnig for having been unfortunately misled by the unknown error of Appunn's instrument to attribute that error to him. Besides this acoustical acceleration of the beats,' he continues, 'there remain two other drawbacks to Appunn's Tonometers: First, they do not retain their pitch with accuracy, and secondly, their variation with temperature is unknown. Hence they are not, as I had hoped, instruments of scientific precision, though admirable for all purposes of lecture illustrations.'

The remainder of the paper, being mainly concerned with historical

details, hardly comes within the province of Physics. It is, however, a most laborious and exhaustive discussion of a somewhat neglected subject.

Measurements in Electro-optics.—Dr. Kerr continues this important subject in the *Philosophical Magazine*. He finds that the dioptric actions of dielectrics are generally of one kind—pure double refractions with reference to the line of electric force as axis; but they vary largely in intensity, and even in sign, from one dielectric to another. Dielectrics are optically equivalent to uniaxial crystals, and exhibit like variations, both from strong to weak, and from positive to negative. Carbon disulphide is of the positive class, and in regard to strength holds a place like that of Iceland spar among crystals.

The law is most probably as follows. The intensity of electro-optic action of a given dielectric, per unit thickness, varies directly as the square of the electric force.

He describes at length a new form of cell in which a 4-inch plate of powerfully refractive and dispersive liquid is enclosed between two conductors of brass, the lower resting on its floor, the upper supported by a glass rafter. The opposed faces are flat and smooth, parallel, and distant about $\frac{1}{12}$ in. All edges and corners are rounded away. The cell is filled with about a pint of clean disulphide. Two Nicol prisms are placed in the path of a beam from outside, the ray passing between the conductors in a slit four inches long, one inch broad, and $\frac{1}{12}$ in. deep, the latter lying vertically as the line of force. Wires are led from the lower conductor to earth, and from the upper to the prime conductor. On starting the machine, the potential of the upper conductor rises slowly, and the black space between the conductors is gradually illuminated, passing from black, bluish-grey, faint white, to a pure and brilliant white. Although the highest potential reached is comparatively low, the optical effect is very large, and beyond neutralization by a band-compensator of strained glass. As the potential rises, the polariscope gives a fine progression of chromatic effects, which descend regularly and continuously through a certain range of Newton's scale. The luminous band passes from white to bright straw-colour, deepening to yellow, then through orange to deep brown, then to pure and dense red, then to purple and very deep violet, then to rich and full brown, then to green. About the last point the process generally terminates in spark discharge. The dielectric acts like a plate of quartz with optic axis parallel to the lines of force, and increasing in thickness rapidly as the potential rises.

To measure potential he uses a Thomson's long-range electrometer, and a Jamin's compensator with the axis of its prisms perpendicular to the ray, one vertical, the other therefore horizontal. A convex lens gives a distinct view of the black band and the reference-wires in the compensator. The exact process of measurement requires much detail for its description. It gave congruous results. In conclusion, the law of squares is stated in several forms. The quantity of optical effect varies: 1. Directly as the square of the resultant electric force; 2. directly as the energy of the electric field per unit of volume; 3. directly as the mutual attraction of the two conductors; 4. directly as the electric tension of the dielectric: a quantity long ago conceived clearly by Faraday, and introduced into theory by Clerk Maxwell. Their views are strongly confirmed by the new facts.

A Glycerine Barometer.—Mr. James Jordan communicates to the Royal Society the details of this instrument. Many attempts, as he states, have been made from time to time to construct barometers with fluids of lower density than mercury, with the view of increasing the range of oscillation. He expresses the belief that such instruments may show the character of more minute vibrations of atmospheric pressure at storm stations. Many have been made with water, notably one in 1830 at the Royal Society, by Prof. Daniell. These, however, are vitiated by the effects of change of temperature on the water vapour in the vacuum, which marks changes of pressure. Glycerine, from its high boiling-point, has a very low tension of vapour at ordinary temperatures, and a very small coefficient of absolute expansion. The specific gravity of the purest glycerine is 1·26, less than $\frac{1}{10}$th that of mercury. The mean height of the column is 27 ft. at the sea level; a variation of $\frac{1}{10}$ in. in the height of the mercurial column is equal to more than an inch in glycerine. As it is very hygrometric its surface is covered by a shallow layer of heavy petroleum oil.

The tube is formed of ordinary composition metal pipe of $\frac{5}{8}$ in. internal diameter. To this is cemented at the top a glass tube 4 ft. long, with inside diameter of 1 in. The upper end is formed into an open cap fitted with an indiarubber stopper. Two scales, one on either side, read off the height, one being divided into inches and tenths of absolute measure, the other into equivalent values of mercury.

The cistern is cylindrical, of copper tinned inside, 5 in. deep and 10 in. diameter, with a cover and small orifice covered with cotton wool to keep out dust. Glycerine, coloured red by aniline, was heated to 100° Fahr. and placed in the cistern; by means of an air-pump connected with the top of the tube the level was raised 323·571 inches, or within ·3 of the Kew standard. A plug was then screwed in below to support the column, the tube was filled at the top with glycerine, and the stopper inserted. Some precautions were adopted to allow air to escape, and the column was finally allowed to take its own position. It will be regularly observed by the Superintendent of the Observatory.

Accidental double Refraction forms the subject of a communication to the *Annales de Chimie et de Physique* by M. Macé de Lépinay. He distinguishes four kinds: 1. By pressure or tension, discovered by Brewster. 2. Lamellar polarization, due to cubic crystalline structure. 3. Results of unequal temperature or chilling. 4. Double refraction from high electrical tension, as discovered by Kerr. The first (1) is simply proportional to the force employed, starting with a certain value, and leads to some remarkable results; for instance, quartz cut perpendicularly and compressed gives lemniscate curves, thus becoming biaxial for the time. Under the second (2) come the experiments of Seebeck in 1812 and Brewster in 1814, which have been revived with practical purpose of late by M. de Bastie in his toughened glass. These are clearly due to excessive surface tension. The writer of the memoir goes into long and careful experiments as to (*a.*) the distribution of stress in rectangular plates; (*b.*) the verification of Wertheim's law of wave-lengths; (*c.*) the variations of ordinary and extraordinary indices in various points of rectangular plates. The paper itself extends to ninety pages, and contains, besides the direct objects of the inquiry, some valuable collateral

matter, such as a method of verifying Jamin's compensator with great delicacy.

Distinguishing Lights for Lighthouses forms the subject of a communication from Sir W. Thomson to the *Times*. He recommends: (1) A great quickening of all revolving lights. (2) The application of a group of dot-dash signals to every fixed light. (3) The abolition of colour as a distinction for lighthouse lights, except for showing dangers, channels and ports by red, white, and green sectors. Of about 120 revolving lights on the English, Scottish, and Irish coasts, there are in all eighteen in which the periods are ten seconds or less, and the times of extinction seven seconds or less. In these quick revolving lights, the place of the light is not practically lost during darkness; the eye, sweeping deliberately along the horizon, with or without the aid of a binocular, 'to pick up the light,' passes over less than its own field of view within the period of the light, and thus finds it almost as surely as if it were fixed. What a contrast to the ordinary minute-period revolving light!

The distinction by colour alone ought to be prohibited for all lighthouse lights, on account of its liability to be confused with ships' and steamers' side-lights. Southsea Castle, with its red and green port and starboard side-lights, seems as if actually planned to lure an unsuspecting enemy to destruction. His proposal is to distinguish every fixed light by a rapid group of two or three dot-dash eclipses; the dot of about half a second duration, the dash three times as long, with intervals of light, about half a second each, between the eclipses of the group, and of five or six seconds between groups.

Siemens's Differential Electric Lamp has one carbon attached to the end of a lever joined to a pair of iron cores, which are free to move up and down in two solenoids. One of these has large wire of small resistance, forming part of the lamp circuit. The other is a coil of smaller wire, offering greater resistance. It is in a circuit external to the lamp, joining the conductors and excluding the carbons. When the former is excited, it draws in its core, and the points of the carbons are separated; when the latter, they approach one other. The distance will thus be adjusted automatically, so as to maintain constant action.

Connection of Surveys has been recently accomplished by M. Perrier, between Algiers and Europe. He found that from all the trigonometric points of first order about Oran, the loftier peaks of Sierra Nevada were visible in clear weather. The stations chosen in Algeria were the summits of Mount Filhaoursen and Mount M'Sabiha, west of Oran; in Spain, those of Mounts Tetica and Mulhacen—the latter the most elevated point in the kingdom. The signals were to have been given by means of solar reflectors and powerful lenses, over a distance of 270 kilometres, but they failed utterly. Preparations had, however, been made for the employment of the electric light, and on the summit of each mountain a Gramme's machine had been established. On August 20 the lights were displayed all night. It was not until after twenty days that one after another they became visible even to the naked eye. That on Tetica, nearly 270 kilometres distant, about equalled α Ursæ Majoris, which rose near it. We have now trigonometric measurements of an accurate nature from lat. 61° in the Shetland Islands, to lat. 34° on the southern frontier of Algeria.

A New Standard of Light has been suggested by Mr. Louis Schwendler, in the *Journal* of the Society of Bengal. It consists of a U-shaped piece of platinum foil, about 20 mm. in length, each limb being 3 mm. in breadth, and at the top with metal clips: a current, the amount of which is registered by a galvanometer in circuit, is passed through this. It is a step in advance of the candle standard, though not of the same accuracy as other scientific units.

Coloured Rings on the Surface of Mercury have been obtained by M. Guebhard, by clearing it of oxide and breathing on it. They contract as evaporation diminishes the thickness. The best results are obtained with collodion, diluted by means of ether. These pellicles can be detached and transferred to paper.

ZOOLOGY.

A Synthetic Starfish.—Under the name of *Astrophiura permira*, Mr. W. Percy Sladen has described (*Annals and Mag. Nat. Hist.* December, 1879) a most remarkable form of Echinoderm from the coast of Madagascar. While the ordinary starfishes present usually the well-known star-like form, with five or more rays springing from a central body with which they are perfectly continuous, the body in the Ophiurids is a rounded or more or less pentagonal disc, from which issue five jointed arms, quite distinct in structure from the disc and from the much stouter rays of the ordinary starfishes. Mr. Sladen's new form combines the characters of the two groups in a very singular manner; and curiously enough, it is towards the somewhat aberrant forms of starfishes (such as *Goniodiscus*) in which the enlargement of the disc and shortening of the rays converts the whole body into a pentagonal disc, that the new type seems most to approximate in outward appearance. In fact, the arms are for the greater part of their length enclosed in a disc formed of calcareous plates both above and below, but a small portion of jointed arm projects from each angle of the pentagon thus formed, and with the structure displayed along the lines of the arms on the lower surface sufficiently demonstrates the Ophiuridan affinities of the organism. By careful study indeed Mr. Sladen makes out that the whole skeletal structure is due to an abnormal development of the ordinary plates of an Ophiurid; but at the same time he recognizes in the structure of the animal a number of characters which tend towards the Asteroida, such as a great development of the ambulacral system, with formation of supplementary plates separating the tentacular compartments, the extension of the peritoneal cavity into the radial portions of the animal, and the organization of the mouth. Mr. Sladen's paper, which is illustrated with an excellent plate, deserves the attention of all zoologists.

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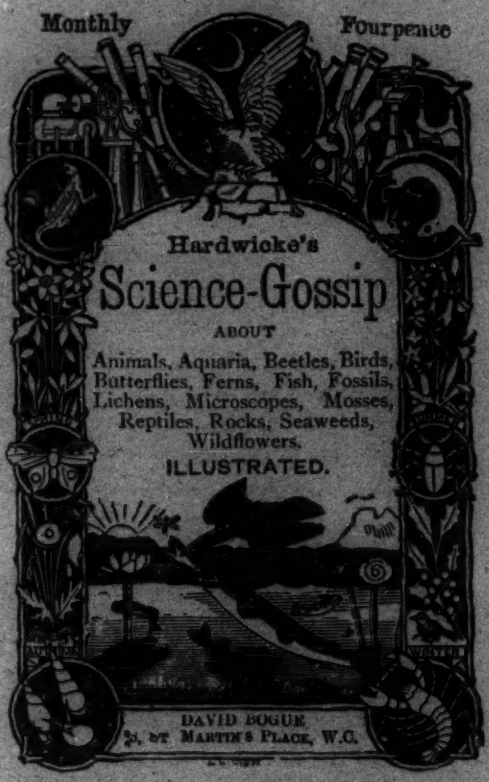

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